LINGUISTIC RECONSTRUCTION: ITS POTENTIALS AND LIMITATIONS IN NEW PERSPECTIVE

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Henrik Birnbaum

University of California, Los Angeles

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SUMMARY

The paper sets out to redefine and explore the methods of linguistic reconstruction, its functions and constraints. Specifically, the issue of a more precise definition of internal reconstruction, implying a procedure of "projecting backward" a set of synchronically ascertained data and processes of one language, is addressed, and a more stringent delimitation of internal reconstruction vis-à-vis the use of the comparative method for the purpose of reconstruction is proposed. In addition to the two chief traditional approaches for recovering lost stages of linguistic evolution, some further methods, aimed at either combining these two techniques (the 'integrated comparative method') or supplementing them ('external reconstruction' based on extra-systemic language data provided by loanword and onomastic material as well as extra-linguistic evidence), are discussed and their merits and shortcomings assessed. The drawbacks of lexicostatistics/glottochronology, which has been proposed as a further tool in linguistic reconstruction, are noted. The structural affinity of reconstruction and prediction, and the once claimed parallelism between linguistic diachrony and the order of synchronic rules in transformational-generative language description are briefly commented upon. The potentials and limitations of a modeling approach to linguistic reconstruction, superimposing, as it were, related-language structures for the purpose of arriving at new insights concerning their relationship (esp. chronology), are discussed. The specifics of reconstruction at various levels of linguistic structure — phonological, morphological, syntactic, and semantic — as well as the very nature of linguistic change are reexamined. The data and references used to illustrate the above theoretical considerations are primarily drawn from Indo-European, notably Slavic, but some suggestions are also made regarding the possibility of ascertaining, by 'external comparison' (genetic and/or typological), distant linguistic relationships (or 'macrofamilies') and their underlying parent languages ('preprotolanguages'), or an abstract model thereof, such as those of the redefined 'Nostratic' languages of the Old World.

0. Preliminary remarks: diachrony and synchrony in reconstruction; traditional and generative approaches

Problems of linguistic reconstruction, that is, of the partial or complete recovery of unrecorded protolanguages as well as lost (nonattested or unknown) but retrievable stages of extant or extinct languages by means of a set of suitably devised techniques, have long preoccupied students of historical and comparative linguistics. In more recent years, attention has also been given to these problems by some adherents of primarily synchronically oriented generative grammar of one brand or another. [1]   Generally speaking, several approaches to linguistic reconstruction can be identified, each with slightly different goals. Thus, it is common to distinguish between such reconstruction as can be attained by applying the comparative (or, more precisely, historical comparative) method, on the one hand, and that which, by projection backward in time and by focusing on the establishment of relative (rather than absolute) chronologies, operates exclusively on a synchronically — often meaning currently — available set of linguistic data, on the other. The latter methodology is also known as internal reconstruction, especially if its input data is drawn from only one language, [2]   and it is to this particular approach that generative grammarians have on occasion laid some claims. They have done so by suggesting a far-reaching parallelism between, if not outright identity of, the outcome yielded by the methods used in internal reconstruction and that resulting from procedures designed for the formulation of generative, including transformational, rules at one level of linguistic structure or another. As is well known, these rules are thought to produce the — to be sure, synchronically conceived — "derivational history" underlying (but not preceding) the sequence of the phonetic and, secondarily, graphic or otherwise perceptible output

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1   Cf., e.g₀, King 1969: esp. 154-87 ("Reconstruction"); Stockwell & Macaulay 1972: esp. 22-31 (C. -J. N. Bailey, "The Integration of Linguistic Theory: Internal Reconstruction and the Comparative Method in Descriptive Analysis"); and several studies by P. Kiparsky.

2   On the condition that for purposes of internal reconstruction the diachronic inferences be drawn from the synchronic data of one language only, see, *inter alia*, Birnbaum 1970a: 95-6, with nn. 8 and 10 (citing definitions of internal reconstruction formulated by J. Kuryłowicz).

representing a sentence or some larger segment of a given corpus of utterances.

It is certainly not my intention here to question each and every value and the validity of generative grammar, especially as regards some of its more recent, semantically based approaches and further developments (represented by linguists of in part such differing persuasions as Charles Fillmore, Wallace Chafe, George Lakoff, James McCawley, W. J. Hutchins, Manfred Bierwisch, Igor' Mel'čuk, Jurij Apresjan, Renate Bartsch, and Theo Vennemann). Yet, in many instances, the criticism leveled at some of the more outlandish ideas and claims advanced by the originator of transformational-generative (TG) grammar, Noam Chomsky, and the supporters of the ever-shifting and, at the same time, increasingly refined 'standard theory' seems justified. Two telling examples will suffice to indicate some of the areas where I, along with others, find it difficult to follow the reasoning of the advocates of that orthodox doctrine. One is the insistence on certain allegedly innate but, as it turns out, full-blown linguistic abilities in man (and not just the assumption of the inborn potential or neurophysiological prerequisite to develop linguistic skills in the course of first-language acquisition). Another one is the explicit denial of any significance whatsoever accorded to directionality in the process of generating surface structures (and not merely the recognition that the linguist in his generative description obviously is at liberty to take as his point of departure any of the structural strata of language, with the phonetic and/or syntactic surface structures sometimes providing clues relevant to syntactic and/or semantic deep structures). By the same token, however, it seems to me unwarranted to unqualifiedly dispute — as is now increasingly often done — the very foundation upon which the whole generative concept of language and its functioning rests (a concept adumbrated, it should be remembered, by W. v. Humboldt's view of language as *energeia*, with the *ex post facto* realization of this lineage in linguistic thought by Chomsky and his associates). I am referring here, of course, to language as an information-processing and -transmitting system, not to its other, aesthetic function — limited to poetic language — as verbal art. Still, I, for one, consider not only unprovable but intrinsically false any claim or even suggestion to the effect that it is plausible to expect and indeed possible to ascertain

a more than purely fortuitous parallelism or, what is more, an inherent total coincidence of, on the one hand, the diachronic processes of linguistic change (and the formulae elaborated to symbolize them) and, on the other, the set of rules formulated (or processes they reflect) to account for a generative, i.e., formal and explicit, description of surface structures, phonetic as well as syntactic, along with the underlying, deep-seated ("covert") data of a given language and its well-formed sentences. 3   To be sure, an apparent but deceptive and hence irrelevant semblance of such a parallelism or coincidence sometimes may arise, namely, whenever diachronic changes (and, in particular, sound shifts entailing morphophonemic alternations) can be shown to have followed the rationally most direct and thus maximally efficient (in a technical sense, i.e., economic) path. 4

1. Linguistic structure: entities, levels, processes

Minor differences of opinion aside, it is assumed here that many if not most contemporary linguists could agree on the very essence of linguistic structure (or, in the abstract, of a model of language) and its functioning. The latter can be viewed as the correlation and interaction between a set of identifiable discrete entities molded from the — prior to linguistic processing, amorphous — universe of meaning and a concomitantly isolated set of discrete units of the initially unbroken spectrum of sound. While these sets of semantic and phonological "building blocks" of linguistic structure must be regarded as syntagmatically (sequentially) minimal and thus not

3   For some arguments along similar lines, see also the section "Internal Reconstruction, Order of Rules in Generative Grammar, and the Problem of Early Balto-Slavic Relations," in Birnbaum 1970a: 92-122, esp. 99-113. For my concept of stratified deep structure, see Birnbaum 1975c and 1977.

4   I therefore strongly disagree with the opinion formulated in Halle 1962/4: esp. 67-8/347-8, summing up the author's then held view on this matter. By the same token, I can only subscribe to the opinion expressed by J. Kuryłowicz when he states: "Such an approach [viz., the techniques of linguistic reconstruction, H.B.] is different from that of generative grammar whose analysis may *accidentally* [author's italics] coincide with the results of historical reconstruction," and adds in a footnote: "Especially in cases where relatively short and recent prehistorical or preliterary periods are concerned." (Kuryłowicz 1973: 63) For some qualification of this rejection of the claim made by generative grammarians, see below, in section 4.

susceptible to further analysis (segmentation) in that — i.e., the linear — dimension, they can be broken down into yet smaller units in terms of their paradigmatic (cooccurent)makeup. These ultimate elements, finite in number (quite large, though, as regards meaning, relatively small when it comes to sound), are referred to as features, distinctive (emic) or redundants(etic or emic). The minimal segments of meaning and sound can thus be said to form compounds ("bundles") of semantic and phonological features fused by a process of automatized selection. The above mentioned interaction between these smallest discrete units of meaning and sound is implemented by a set of rules (or, in another terminology, functions) residing in what figuratively can be labeled various strata or levels of linguistic structure intermediate between articulate meaning and segmented sound and usually referred to as syntax and morphology. Within each of these layers or components of grammar it is, again, possible to differentiate between a (relatively) more abstract or "deeper" level and a (relatively) less abstract, i.e., more "shallow" or "superficial," level of representation. It is further at least my contention that the semantic component of language informs the syntactic one while surface syntax in turn determines the sequential arrangement of the morphological units and these in turn impart the by then fractionated bits (in the communication-theoretical sense) of information to the relatively most abstract segments of the phonological component, the ("systematic") phonemes, *alias* morphophonemes (in traditional, roughly equivalent usage). The conversion of this abstract signal to the actual sound of the phonetic string (speech chain) is then carried out by a set of phonological processes (framed in terms of rules) operating on the nervous system and articulatory (physiological) apparatus of man. By contrast, as is known, syntactically-oriented traditional generativists of the MIT school maintain that only the syntactic component, assigned a central position in their model of linguistic structure, is, strictly speaking, generative ("creative") while both the phonological and semantic components are merely interpretive, i.e., serve to map sound-related and meaningful elements onto the syntactic structures generated by the syntactic component (or, *vice versa*, relate the "slots" established in surface syntax to the discrete elements of sound and meaning).

This exceedingly brief sketch of the functioning of language as a coherent structure, in the view adopted here, was necessary by way of general background. It is against this background that we can proceed to expound in some detail the specific strategies of reconstruction applicable to various structural levels of language — semantics, syntax, morphology, and phonology (including in the latter also morphophonology or morphophonemics) — with further internal stratification within each of these components contingent upon degrees of abstraction and complexity, i.e., relevance to wholes and/or parts peculiar to each of the just listed domains of language. In this context it should be noted that one, pragmatically in most instances readily identifiable but theoretically not easily defined, linguistic unit, crucial to all reconstruction, the word, legitimately belongs as an integral entity to more than one level, viz., the semantic — as a carrier of meaning (i.e., lexeme, consisting of one or more sememes), and morphological — as a formation subject to the rules of derivation (or, more generally, word-formation) and inflection (i.e., "word form" in the sense of Russian *slovoforma*).

2. Methods of reconstruction

Before we go into some specifics of the methods of linguistic reconstruction at various levels, a few general remarks need to be made about the way in which the comparative method of reconstruction and the techniques of internal reconstruction, though clearly distinct and different, can complement each other or even be merged and integrated into one complex methodology in order to achieve optimal results. Moreover, worthy of mention are some further procedures and devices of reconstruction, involving, among other things, juxtaposition and superimposition of fairly abstract linguistic models. In addition, the linguist concerned with reconstruction can draw on extra-systemic language data. This data includes evidence provided by loanwords and onomastic, notably, toponymic and hydronymic, material. The latter, in particular, frequently preserves archaic, "frozen" phases of phonological and morphological evolution. In a similar vein, specimens of folkloric language use occasionally may retain syntactic structures and

phraseological turns long lost in attested stages of a literary language; the same can also apply to no longer extant meanings of individual words. Furthermore, extra-linguistic information, obviously, often bears on both absolute and relative chronology of linguistic change. Finally, a word or two must be added by way of preliminaries concerning the sometimes invoked significance of lexicostatistics and/or glottochronology for purposes of linguistic reconstruction which in my view is by and large questionable or at any rate exaggerated.

In his meticulously researched, insightful attempt at tracing the Common Slavic sound system and its unrecorded evolution (undeservedly, in my opinion, criticized in overly harsh terms for not having adopted more or less modern methodologies, such as distinctive feature analysis or generative phonology), George Shevelov discusses, by way of introduction, methods of reconstruction and relative chronology. He contends that, at least as regards diachronic phonology, the comparative method loses its atomistic character if enriched by the set of tehniques devised to capture relative chronology which, as we know, is pertinent especially to internal reconstruction. [5] As for the latter, the American Slavist has this to say: "Taking a language as a system in which everything which is alive and productive must be motivated, it [i.e., the method of internal reconstruction, H. B.] considers non-motivated elements of the language as petrified remnants of older stages in its development. This method, invaluable in historical morphology, is of much less importance in phonology. However, it can also offer phonology some glimpses into the past." [6] One can argue, of course, that by the fact alone that relative chronologies in large part are establishable only as a result of applying the method of internal reconstruction, the latter's significance also for diachronic phonology is beyond doubt. Writing on a related subject matter, viz., the reconstruction of the various components of Common Slavic linguistic structure, I stated the

5   See Shevelov 1964/5: 6-9. For the procedure combining the comparative method and the approach establishing relative chronologies for purposes of linguistic reconstruction Shevelov uses the term "integrated comparative method" (ICM).

6   Shevelov 1964/5: 5. Cf. there also further: "Both the comparative method and the method of internal reconstruction make the reconstruction of older stages in linguistic development possible, but they do not guarantee any reconstruction of an older language as it really existed at a certain time .... Both methods are essentially historical but timeless. They project various developments on one surface."

following in an opening section of my recent account of the
progress made and the problems remaining in the recovery of
the Slavic protolanguage: "No direct evidence is available
on which to base any assumptions as to the phonological and
grammatical (morphosyntactic) structure and the basic vocabu-
lary of the fairly homogenous development of C[ommon]
S[lavic] prior to ca. 500 A. D. Virtually all attempts at recover-
ing these earlier stages of the Common Slavic protolanguage
must therefore rely heavily on the methods of internal recon-
struction, i.e., on techniques by which data of the last phase
of an already disintegrating and dialectally differentiated CS of
ca. 500-1000 A. D. can, as it were, be projected backward in
time. This allows for some inferences from morphophonemic
alternations, competing word forms, and coexistent syntactic
structures of this late stage of CS as to primary (vs. secondary
or partly even tertiary) sounds, forms, and, at least to some
extent, also phrase, clause, and sentence patterns, suggesting
the establishment of certain relative chronologies pertinent
to CS linguistic change. ..... The validity of the results thus
obtained can subsequently be verified in many instances by
correlating the hypothetic primary data of earlier CS ......
with corresponding evidence from other IE languages, in
particular those more or less closely related to Slavic ...... Thus
the methods of internal and comparative reconstruction can be
used here to supplement each other and to corroborate each
other's conclusions. The structure of disintegrating Late (post-
5th century) CS can in turn be reconstructed on the basis of
comparative evidence drawn from the individual recorded Slavic
languages, particularly in their earliest attestation." 7

As for the prospects of retrieving the details, including the
several transitional stages, of the morphological structure of a
nonattested (proto)language by means of the comparative
method or by internal reconstruction, I would think that one
should not be overly optimistic. For what often can be achieved
here is no more than to ascertain the relevant data at the point
of departure for the evolution of a given, not directly observ-
able language and its final, preliterate phase. The chronological-
ly intervening, consecutive periods may, by and large, remain
impossible to recapture. Thus, as applicable to the prehistory
of Slavic morphology, I wrote: "Unlike phonology, the CS

7   See Birnbaum 1975a: 9-10.

morphological system can be reconstructed with some prospect of success essentially only in the very earliest and the latest phases of its development, in other words, in the states of its emergence and ultimate disintegration, while details of the intervening evolution must largely remain conjectural at best. For it is only a relatively insignificant portion of the reshaping of the morphological structure as still discernible in the early recorded evidence ..... that can be assumed to reflect major modifications of the form system in earlier prehistoric, i.e., pre-Late CS, times ...... Early P[roto-] S[lavic] morphological structure .... can be recovered — to be sure, only fragmentarily — by combining techniques of the comparative method applied to IE linguistic data and of internal reconstruction. The latter implies that Late CS data is being projected backward in time, thus filling in the blanks, as it were, in the overall picture established on the basis of comparative evidence from other IE languages. In brief it can be stated that in any attempt at reconstructing the morphology of Slavic linguistic prehistory a polarization, focusing primarily on the initial and final phases only, is inevitable." 8   Subsequently I could only concur with Henning Andersen who had warned: "The student of the prehistorical morphology of Slavic faces formidable problems .... as a consequence of regular phonological developments in prehistoric Slavic ..... the Slavic data alone are not sufficient for the reconstruction of a Proto-Slavic ..... system. Such a system can be posited on the basis of other Indo-European languages; but this reduces the study of Slavic prehistoric morphology to the task of reconciling the attested desinences with what can reasonably be posited for Proto-Slavic and postulating ad hoc changes for those difficult desinences whose attested forms are not directly related to their supposed Proto-Slavic etyma by general, regular phonological change." And I went on to elaborate this particular dilemma by stating: "..... there is a vast gap separating the readily recoverable word form system of Late CS (as it can be reconstructed on the strength of the attested Slavic, especially Early Slavic, evidence ....) and the initial phase of Early PS, just emerged from dialectal Late PIE (or possibly a common Balto-Slavic linguistic structure)." 9   I found myself, therefore, at variance with

8   Birnbaum 1975a: 150-1; cf., for an elaboration of some of the arguments set forth here, also Birnbaum 1973.
9   Birnbaum 1975a: 272-3.

Shevelov's view about the value of internal reconstruction for
the recovery of the diachrony of an unrecorded or lost (proto)-
language, here Common Slavic, when claiming: " ...... while
the polarization between the Early PS and late CS state of
affairs is considerable, the method of internal reconstruction
providing techniques for establishing relative chronologies and
recovering lost evolutionary phases is poorly developed and
quite inadequate when it comes to morphological change." [10]
Yet I could point to at least a modest measure of progress made
in recent years by some linguists who have introduced new,
previously untested techniques to this field of inquiry.

It goes without saying that what has been said here with
regard to the situation in Common Slavic (the particular proto-
language with whose problems I am most familiar) applies,
mutatis mutandis, equally to the possibility (or impossibility)
of reconstructing the morphological changes of other, only in-
directly known protolanguages. In this connection, it may be
worth pointing out that the general validity of the various
approaches to recovering protolanguages to some extent can be
verified or refuted by applying the very same techniques of
reconstruction to those protolanguages that are actually
attested but treated as if unrecorded, i.e., without first taking
into account our direct knowledge of them. Once the results
of such reconstruction, arrived at on purely hypothetic grounds,
are available, they can then easily be confronted with the
actually known data of these recorded protolanguages. The
textbook example of a language suitable for such testing is
Latin with its own long and well documented history and its
simultaneous status as protolanguage for the Romance lan-
guages; for an experiment along these lines, cf. Ferguson 1976.
But analogous considerations and procedures could also be
applied, for example, to the ancestral language of the Nordic
(Scandinavian) languages, Proto-Nordic or Common Norse,
known by the testimony of Nordic names and isolated lexical
items recorded by some ancient authors, in the form of loan-
words in Finnish and Lappish, and, above all, by a number
of Runic inscriptions in the older, 24-sign futhark. And, in a
similar vein, the efficiency and scope of specifically internal
reconstruction could be explored by applying its techniques
to the demotic (*dimotiki*) variety of Modern Greek or, for that

10 Birnbaum 1975a: 273.

matter, to any evolutionary stage of medieval (Byzantine) Greek. (The other variety of Modern Greek, the learned and in part artificially archaizing written language known as *katharevusa*, which was created in the second quarter of the 19th century and is macaronic in character, is less suited to this purpose.) The results thus obtained can then be checked against the abundantly attested data of ancient (classical) Greek, including its early Homeric form, or, to the extent possible, even the scanty noncontroversial evidence of Mycenaean Greek, recorded in Linear B. This uniquely represented branch of Indo-European has a more than three thousand year-long known history and therefore lends itself particularly well to this kind of testing.

As was indicated above, an additional method that can prove useful in establishing chronologies, relative as well as absolute, and provide further insights into the structure of unrecorded (proto)languages is one which could be termed the modeling approach. Here the linguist, after first having designed, by means of comparative and, in particular, internal reconstruction, more or less complete models of the structural — or at least the phonological and morphological — characteristics of two related languages which are not directly observable, then proceeds to compare and contrast these two models with each other. He can do so by juxtaposing them and, for even more immediately telling results, superimposing, as it were, one over the other, thus readily identifying shared (vs. non-coinciding) features and establishing chronological layers. Precisely such a procedure was proposed and sketched several years ago by Vjačeslav V. Ivanov and Vladimir N. Toporov for the purpose of shedding more light on the nature of the relationship of Slavic and Baltic or, to be more precise, the earliest reconstructable phases of these two branches of Indo-European, Proto-Slavic and Common Baltic. (Some doubts remain, however, concerning the actual existence of the latter as a homogeneous linguistic reality; rather, perhaps, Common Baltic can be viewed merely as the theoretical sum total of a number of exclusively shared features of the recorded Baltic languages.) As a result, the two Soviet linguists could further corroborate the previously held view claiming the more archaic character of Baltic as compared to Slavic. They suggested that, in many instances, it is possible to derive entire sets of Slavic

sounds and forms from a pre-Slavic (i.e., Balto-Slavic) prototype essentially identical with a hypothetic model of Common Baltic (or dialectal Late Proto-Indo-European underlying Baltic); as they were able to show, a reverse procedure, taking the Proto-Slavic phonological and morphological system as a point of departure for the subsequent Baltic development, would not produce acceptable results. 11   It would seem that analogous techniques could be applied to other pairs or groupings of partially recoverable protolanguages for which a particularly close relationship, presumably reflecting a shared (or parallel) post-Proto-Indo-European evolution, has been assumed. This would thus be true for Indo-Iranian and possibly also for some other branches of Indo-European, such as Italo-Celtic (whose common post-PIE origin however now seems less certain), Italo-Venetic, and perhaps even for Anatolian and Armenian (as well as, conceivably, Thracian or Phrygo-Thracian and 'Pelasgian', one of the pre-Greek substrata of prehistoric Greece). 12   Outside Indo-European, similar subgroupings could be singled out and thus compared, for example, sub-groupings within the Finno-Ugric and Altaic language families.

It should not be necessary to elaborate here at any length on some of the other points mentioned above. Among them is the fact that extra-systemic language data, especially loanword and onomastic material, frequently can provide additional valuable information germane to the reconstruction of un-recorded protolanguages as well as of lost stages of otherwise known languages. Likewise, it would appear readily clear that archaic language usage as it is occasionally preserved in texts — including poetic texts — of a folkloric nature can contribute to a fuller and more correct grasp of the syntax, phraseology, and semantics of languages or phases of linguistic evolution for which no direct evidence is available. Nor, it would seem, does one need to specially belabor and illustrate the point that extra-linguistic data, as provided by historical records or known historical facts and, to some extent, by the evidence produced

11 Cf. Ivanov & Toporov 1961: esp. 275-6 and 303-4. See further also my concurring remarks in Birnbaum 1970b: esp. 71-3, and 1970a: 87-8, 113-14 (with fn. 38) and 117-18. For a somewhat different view of the Baltic and Slavic proto-languages as two fully separate branches of Indo-European, see now Mayer 1976.

12 On the possibility of a relatively close relationship between the Anatolian language group and 'Pelasgian' and the potential near-identity of 'Pelasgian' and Thracian, see Birnbaum 1974: esp. 381.

by other disciplines with a historic or prehistoric dimension, can shed more light on problems also of specifically linguistic chronology. Among such disciplines are, for example, archaeology (where, however, the danger of arriving at ethnolinguistic inferences without the corroboration of supporting linguistic data remains very real and a reasoning along archaeological and prehistoric lines alone sometimes tends to obfuscate rather than to clarify a particular linguistic issue) and botany (where modern methods of pollen analysis have supplied revealing data pertinent to the time-depth of botanical nomenclature not otherwise available).

Perhaps, however, a few remarks are in order here regarding the claimed potentials of lexicostatistics or glottochonology, a method launched by Morris Swadesh, which operates primarily on lexical and morphological entities. In my view, the underlying supposition of a stable (or, at any rate, predictable) rate of linguistic change susceptible to statistical interpretation as far as the loss (and, conversely, retention) of morphemes and whole lexical items is concerned must be considered essentially false as it is controverted by factual evidence. In this context it should be remembered that the procedures developed for lexicostatistics were, to some extent, patterned on the model of measuring the rate of redioactive decay of perishable matter by the, as it turned out, somewhat less than precise (and subsequently rather drastically adjusted) techniques used for radiocarbon (carbon 14) dating of archaeological material. But this analogy is, in fact, not viable and I, for one, do not find, therefore, that the procedures of glottochronology (or of the somewhat broader domain of lexicostatistics) as we now know them constitute anything near a valid and reliable tool usable in linguistic reconstruction, comparative or internal. In other words, I do not find that it has been possible to ascertain a constant (or even fluctuating, within a predictable range) rate of vocabulary turnover or invariant percentage of morpholexical retention and thereby determine chronological linguistic relationships which are otherwise not establishable. 13

13 For a general presentation of glottochronology/lexicostatistics, see, e.g., Lehmann 1962: 107-13. For a critical assessment, cf. further Fodor 1965; and, in particular, Anttila 1972: 395-8. All above references cite additional pertinent literature. Anttila does not equate glottochronology with lexicostatistics but considers the latter the broader field of inquiry which includes the former. Specifically, he distinguishes three uses of lexicostatistics, 1) time depth (=glottochronology), 2) sub-

3. Diachrony: reconstruction and prediction; Grouping: genealogy and typology

Before going any further into the problems of reconstruction as they pertain to the various levels of linguistic structure as well as to this structure as a whole (or to the models replicating it in an abstract fashion), something should be said concerning the relationship and structural affinity obtaining between linguistic reconstruction, and in particular internal reconstruction, on the one hand, and, on the other, forecasting or prediction and, specifically, what I have termed 'projective prediction' of linguistic evolutions to come, a topic dealt with by me (and others) on previous occasions. [14]

By and large, I continue to stand by my contentions made on this subject a decade ago and summarized as follows: "The two main aspects of diachrony, viz., the inquiry into the past and into the future of language evolution, can undoubtedly be viewed as two correlative approaches to the same basic phenomenon of time-determined linguistic change. In this sense, then, there is a correlation between genetic linguistics, concerned primarily with reconstructing lost protolanguages and studying language relationship and divergence, and typological linguistics, in particular areal-typological linguistics, focusing on problems of language affinity and convergence areas and rendering possible, to some degree, prediction of future language developments. Once this general and, to be sure, widely neglected correlation of approaches to diachronic linguistics has been established....., it is necessary, however, to state also the limitations and qualifications of such a correlation. Thus, while reconstruction and prediction and, specifically, internal reconstruction and projective prediction, both based on the evidence of synchronic data, can be viewed as two correlative

grouping, and 3) genetic relationship. For my own earlier expressed skepticism, see Birbaum 1970a: 98, and 1975b: 275 and 289. Of recent, far more positive assessments, cf. some of the papers in Dyen 1973: esp. 75-84, I. Dyen, "The Impact of Lexicostatistics on Comparative Linguistics."

14 Cf. esp. Birnbaum 1970a: 71-91 ("On Reconstruction and Prediction: Two Correlates of Diachrony in Genetic and Typological Linguistics" = *Folia Linguistica*, II: 1/2, 1969: 1-17; an earlier, shorter version of this paper was presented at the Xth International Congress of Linguists in Bucharest, 1967; see *Actes du Xe Congrès international des linguistes* ...., III, Bucharest, 1970: 497-503, with discussion). For problems of linguistic prediction, cf. further, e.g., Pulgram 1967, using French data; and, with Slavic examples, Stieber 1969/72/74; Shevelov 1967/71.

sets of techniques and procedures to penetrate into the un-
known spheres of linguistic evolution — one preceding, the
other yet to follow directly observable change of linguistic
structure — it would imply a gross oversimplification to con-
sider reconstruction and prediction merely reversible methods
of diachronic linguistic research." In concluding these remarks,
I suggested that Friedrich Schlegel's paradoxical dictum styling
the historian a prophet predicting backward, cited by Joseph
Greenberg and Roman Jakobson in their pleas for linguistic
typology, striking and appealing as it may seem, is not to be
taken too literally. [15]   For, as we all know, language is not
a phenomenon which, all its uniquely specific, internal
structural laws notwithstanding, evolves freely and without
interference in a historical vacuum. Whatever predictions
a linguist may venture to make regarding the probability of
certain developments occurring in the future course of a
language or a — perhaps converging — language group of a
particular area (belonging in that case to one and the same
Sprachbund), his predictions cannot but remain theoretical
constructs realizable only under "ideal" conditions. In
actuality, however, language always is susceptible to the extra-
linguistic factors of social and political interference. In all like-
hood, considerations of this kind also led Ernst Pulgram to
caution at the end of his essay on the topic of prediction in
linguistics: "Since linguistics treats of human behavior, as do
social sciences, it needs to reckon with both the internal
structural and the external social forces that shape language.
And the linguist can do no more, and should do no less, than
the economist or the sociologist or the political scientist, name-
ly, observe in, and extrapolate from, the available past and
present evidence the indications of a trend, and draw from it
tentative conclusions for the future. But ..... the social
scientist's gaze into the future cannot lead to predictions as
precise and certain of fulfillment as those of the natural
scientist, whose material evolves *sub specie aeternitatis* and lies
beyond human intervention." [16]
    If there is one point in my previous view of the parallelism,
in goals and in methods, of linguistic reconstruction and predic-

15 Cf. Birnbaum 1970a: 90-1; for further references and some reasoning along
the same lines, see also *ibid*.: 71 and 120-2.
    16 Pulgram 1967: 1648.

tion that I would now want to somewhat modify, it is this:
When writing then, I considered genetic and typological lin-
guistics fully equivalent branches of the science of language. As
a consequence, I conceived of reconstruction as falling squarely
into the domain of genetic linguistics only while the forecasting
of future language evolution (particularly within the confines
of a regionally defined *Sprachbund*), to the extent it was
meaningful at all — bearing in mind the possibility, nay, the
high probability of human intervention causing a deviation from
the set course of linguistic development —, was thought to be
the exclusive concern of typological linguistics. Since then I
have come to realize that of the two chief ways of grouping
and classifying languages, genealogy and typology (purely areal
classification being clearly secondary and hence negligible),
the latter must be considered hierarchically superordinated to
the former. Every genetically ascertainable language family is
characterized, among other things, by a set of correspondences
existing between the systems of expression (or symbolization)
elements, i.e., phonological segments in its member languages.
These elements are deducible from the sound system of a
common ancestral language. At the same time, a genetic
language family can be conceived of as constituting a particular
linguistic type marked, in addition to the just mentioned sound
correspondences, also by other agreements (or further develop-
ments of such original conformities) in morphology, syntax,
and/or lexicology. [17]   If this line of reasoning is adopted, it
follows that linguistic reconstruction is not merely one of the
central tasks of genetic linguistics but that, in a broader sense,
it is also a matter to be dealt with from the point of view of
language typology.

4. Phonological reconstruction

It is fair to say that in the postwar era typological considera-

[17] Cf. esp. Birnbaum 1975c and 1975d; see further also my opening remarks
in Birnbaum 1975b: 267 and 269 (in the latter place commenting on R. H. Robins'
discussion of language taxonomies). For some earlier thoughts along these lines,
see Birnbaum 1970a: 40-3 and 48. It should further be mentioned here that Louis
Hjelmslev, although considering genetic linguistics "absolutely exact, and .... perhaps
the most exact domain of the humanities," explicitly ranked, in the final analysis,
typological linguistics hierarchically above genetic linguistics; cf. Hjelmslev 1970:
10-31, 83-96, esp. 89 and 96.

tions in particular have had a major impact on the modification
and further elaboration of the methods of reconstruction at all
levels of linguistic structure. For, as Louis Hjelmslev, in anti-
cipation of the great achievements of typological linguistics put
it, "only through linguistic typology can we hope to understand
what laws govern linguistic change and what possibilities of
change a given linguistic type implies. Only through typology
does linguistics rise to quite general points of view and become
a science." 18   And exactly two decades ago, Roman Jakobson
in his report on "Typological Studies and Their Contribution to
Historical Comparative Linguistics" presented to the Eighth
International Congress of Linguists in Oslo, when discussing
"Typology and Reconstruction," pointed out that " a conflict
between the reconstructed state of a language and the general
laws which typology discovers makes the reconstruction
questionable." And he went on to illustrate his assertion by
himself questioning, among other things, the "one-vowel
picture of Proto-Indo-European," posited by some of the
adherents of laryngeal theory who would explain not only the
quantitative but also the qualitative — i.e., timbre — diversity
of the PIE vowel system ($e, o, a; \bar{e}, \bar{o}, \bar{a}$) preserved especially
in ancient Greek and to some extent in Latin as traceable
to an earlier $e + H$ (i and u being merely allophones of the
semivowels j and w), since such a monovocalic system "finds no
support in the recorded languages of the world." Likewise, and
still with reference to the system of phonemes posited for the
Indo-European protolanguage, Jakobson remarked that "no
language adds to the pair $/t/ - /d/$ a voiced aspirate $/d^h/$
without having its voiceless counterpart $/t^h/$, while $/t/$, $/d/$, and
$/t^h/$ frequently occur without the comparatively rare $/d^h/$," the
explanation obviously being that in the opposition $/t/ - /d/$ it is
the latter that is the marked member (in this instance, for
voicing) and thus not susceptible to being additionally marked
for aspiration without the member unmarked for voicing also
having an aspirated counterpart. "Therefore theories operating
with the three phonemes $/t/ - /d/ - /d^h/$ in Proto-Indo-
European must review the question of their phonemic
essence." 19   It was precisely this, Jakobson's typologically
founded line of argument concerning the PIE system of stops
that provided one of the chief reasons for the Soviet linguists

18 See Hjelmslev 1970: 96.
19 Cf. Jakobson 1958/71: 23/528-9.

Thomas V. Gamkrelidze and Vjačeslav V. Ivanov to suggest
a reinterpretaion of the phonemic substance of this system,
traditionally reconstructed as follows:

(b)	bh	p
d	dh	t
g	gh	k

That is to say, the PIE set of stops has usually been assumed to
have a series of aspirated voiced stops (so as not to complicate
the argument, only one series of velars is posited here instead
of the traditional two or three, i.e., including a palatal and/or
labiovelar series as well). Instead, the Soviet scholars propose
that the PIE stops consist of the following correlated
phonemes:

(p)	bh	ph
ṭ	dh	th
ḳ	gh	kh

(The phoneme in parentheses indicates its weak or even dubious
status in the system since few if any reflexes point to such a
reconstruction; the dot under the phonemes of the first column
of the second set — or, in another notation, used by Gamkre-
lidze and Ivanov, *p'*, *t'*, *k'*, — represents a glottalized articula-
tion.) This is not the place to discuss any possible shortcomings
of the thus reconstructed subsystem of PIE consonantism — a
typologically (but perhaps not otherwise) acceptable alternative
would be to posit, following an earlier, neogrammarian usage,
not only a voice but also a voiceless series of aspirates in the
traditional system of PIE stops (cf. also Szemérenyi 1967,
discussed below in section 9) — or, for that matter, to elaborate
on some of its merits (other than its conforming to typological
expectations). It should be noted, though, that this reinter-
preted set of PIE stops would render the consonant system of
those IE languages which exhibit so-called consonant shift
(*Lautverschiebung*), namely, Germanic, Armenian, and Hittite
or, rather, Anatolian in general, and, I should add, probably also
Thracian and, if viable as a separate branch, 'Pelasgian', more
conservative or archaic than the consonant system of the other
branches of Indo-European which show no traces of such a
shift; in other words, the consonant system of the just
mentioned IE language groups would have to be considered
closer to the original point of departure. 20

20 Cf. Gamkrelidze & Ivanov 1972 and, in a slightly expanded German version,

The significance of typological considerations for linguistic reconstruction, and, in particular, internal reconstruction, was again duly emphasized and illustrated with phonological and morphological (inflectional as well as derivational) data also by Jerzy Kuryłowicz in his more recent discussion of the pertinent problems and applicable methods. However, his discussion, as pointed out by me elsewhere, is lacking in an adequate and precise definition of the very concept of internal (as opposed to external or comparative) reconstruction. 21

In an early section of this paper, I rejected out of hand any claims advanced by followers of the generative school (and particularly of the orthodox, MIT-based 'standard theory') which would suggest anything more than a purely accidental parallelism or coincidence of diachronic processes of linguistic change as they pertain to relative chronology and the synchronic order of rules based on logical inference (with, among other things, due account taken of the simplicity criterion). It is therefore now only appropriate that I qualify my negative attitude in the light of some recent attempts to integrate the generative approach with other procedures and techniques by conceding that in any attempt to recover lost protolanguages or linguistic histories generative phonology can on occasion contribute to — but certainly never replace — our usually fragmentary understanding of phonological change and its relative chronology. To achieve that goal, however, it is necessary to properly account for and define the limitations of the generative approach itself, allowing for such procedures as ordering reversal (as distinguished from rule inversion in the sense used by T. Vennemann) 22   rule insertion (proposed by M. Halle in his 1962/64 paper on generative phonology), reversal of unconditional mergers, and the like. Moreover, these considerations apply for all intents and purposes to phonology (including morphophonemics) only, not to other components of linguistic structure. And, finally, in my view, the generative approach *alone* can never yield significant,

Gamkrelidze & Ivanov 1973. Cf. further Gamkrelidze 1972; expanded English version, Gamkrelidze 1975. For further discussion, see Hopper 1973 and forthcoming; Birnbaum 1975c: 13-15 and 1975d: 229-31 and 241. On Thracian and 'Pelasgian', along with Germanic, Armenian, and Anatolian, as languages with consonant shift, see, e.g., Birnbaum 1974: 380-1.

21 J. Kuryłowicz 1973; for a critique, see Birnbaum 1975b: 273-5.
22 Cf. Vennemann 1972.

incontestable insights into the relative chronology of sound
shifts. It is of some value, in providing clues, only when
supplemented by chronological data based on historical records
(cautiously as some of these often must be treated, considering
frequent uncertainties of dating and place of origin, ambiguities
as to the phonetic value of a given transcription, and scribal
whims, not to mention the idiosyncracies of individual writers
and poets). Also, wherever possible, the 'longitudinal' methods
of reconstruction, that is, philological dating (in the broad
sense) and internally inferred rule ordering, must be confronted
and combined with 'latitudinal' reconstruction consisting of a
projection from the horizontal plane of spatial (i.e., essentially
dialectal) distribution of synchronic evidence onto a vertical
dimension of time depth, a wider spread of dialectal phenomena
as a rule suggesting greater time depth. Just such an integrated
and highly circumspect methodology or, rather, set of three
combined methods of reconstructing the facts of relative
chronology was recently proposed and cogently argued by
Matthew Y. Chen who illustrates his points, to a large extent,
with Chinese phonological data (and from whom, incidentally,
the contrast of 'longitudinal' vs. 'latitudinal' reconstruction,
along with these terms, was borrowed). [23]   When employed
cautiously and with due recognition of its inherent limitations,
an undogmatic and flexibly applied generative approach can
therefore undoubtedly prove useful also when it comes to
linguistic reconstruction and, in particular, to establishing
relative chronologies of sound change. Yet the remarks just
made should by no means be construed as implying a complete
parallelism or coincidence of the order of synchronic phonolo-
gical rules (of the generative kind) and relative chronology in
diachronic phonology.

[23] See Chen 1976. For my own cautious view on this matter, expressed already
nearly a decade ago, see Birnbaum 1970a: 112-13. The significance of linguistic or
dialect geography or areal linguistics for linguistic reconstruction has of course long
been recognized, by G. Bonfante and others, cf. also the opening, capsule lines of R.
Anttila in the chapter on "Dialect Geography," which, not by chance, forms the
first chapter of Part IV of his thoughtful and erudite text on historical and compara-
tive linguistics, discussing "Linguistic Reconstruction": "The geographical distribu-
tion of linguistic variants may give clues to the relative age of the items. Because
dialect relations have many similarities to reconstructed protolanguages, they throw
much light onto our historical inferences." The remainder of the chapter elaborates
in part on some of the points alluded to in the opening statement. See Anttila 1972:
287-374, esp. 289.

5. *Morphological reconstruction (and the nature of linguistic change)*

It has been suggested above (in section 2) that the linguist working on reconstruction encounters particular difficulties when attempting to retrace the recasting, in successive stages, of lost morphological structure. While this indeed is true in general, a few techniques applicable specifically to the reconstruction of morphological systems ought to be mentioned here. Two of them were discussed and exemplified at some length in a recent paper by Robert Hetzron. The principles involved, though perhaps previously not singled out quite so explicitly, have long been known and resorted to. Following Hetzron, one may call the first of these devices the principle of 'archaic heterogeneity' and the second that of 'shared morpholexical innovations'. [24]   As summarized by Hetzron, the principle of 'archaic heterogeneity' "implies that when cognate systems (i. e. paradigms) in related languages are compared, the system that exhibits the most inner heterogeneity is likely to be the closest to the ancestor-system." As for the principle of 'shared morpholexical innovations', it is said to limit "the scope of the Brugmann-Delbrück principle of shared innovations. The most arbitrary elements of language are the phonetic shape of morphological and lexical items (the requirement of arbitrariness safeguards against possible developments due to general tendencies), and the phonetic shape of morphological items is the least likely to be borrowed (as against lexical items)." [25]   Using Semitic data, Hetzron, elaborating on the principle of 'archaic heterogeneity', states that "if a number of cognate languages each have a system similar to its homologues in the other languages in some respects, but different in other respects — unless one can find a clear conditioning factor for differentiation — the relatively most heterogeneous system might be considered the most archaic, the closest to the ancestor, and the more homogeneous ones might be assumed to have arisen as a result of simplification." Pointing out, to be sure, that "anomaly or irregularity, i.e. heterogeneity in the system, has most often been considered an archaism by those scholars working seriously on problems

24 Cf. Hetzron 1976.
25 Hetzron 1976: 89.

of reconstruction," and citing pertinent statements by Antoine
Meillet and Joseph Greenberg as examples, Hetzron goes on
to suggest that "if the more homogeneous related systems,
which exhibit more internal consistency, differ from one
another, the chances of the heterogeneous one to be more
archaic are even better." [26]

Hetzron emphasizes that "in diachronic linguistics we are not
dealing with full systems, but with the fate of single items or
clósed subsystems, such as paradigms" and takes issue with
Calvert Watkins' suggestion "that 'negative innovations,' i.e.
losing an archaic feature in a number of languages, are just as
important in subgrouping [viz., of related languages, H.B.] as
'positive innovations' introducing new elements." [27]   In this
context, Hetzron mentions as a necessary prerequisite for apply-
ing the method of 'shared morpholexical innovations' that "one
has to find innovations which are as arbitrary as possible as far
as their substance is concerned (which eliminates spontaneous
developments) and as little likely to be borrowed as possible."
To that end, one must look for innovations in the field of
what Hetzron terms "the morpholexicon, i.e. the phonetic
body that makes up a morpheme, a *morph*. By contrast, the
phonetic composition of lexical items, though also arbitrary,
is very easily subject to borrowing while the semantic value
of a morpheme is subject to reinterpretation throughout
history and expecting true identity of functions for proving
relationship is [therefore] not justified." [28]   Cf. further
Louis Hjelmslev's claim that it is on the 'function' (in the
glossematic sense) between the expression elements of the
different languages, or, more succinctly, on 'element-function',
that genetic relationship between languages rests. [29]   Recalling
that "many scholars" — exemplified by Karl Brugmann,
Antoine Meillet, Vladimir Georgiev, and Henry Hoenigswald —
"have expressed themselves in favor of morphology as the
strongest criterion in reconstruction," Hetzron states that
"the preference given to the morpholexicon .... should ......
not be construed as a summary dismissal of the time-honored
phonetically based classification in Indo-European comparative
linguistics and elsewhere" but contends "that morpholexical

26 Hetzron 1976: 93 (with fn. 6).
27 Hetzron 1976: 96-7.
28 Hetzron 1976: 98.
29 Cf. Hjelmslev 1970: 13 and 20.

innovations usually constitute safer criteria for subgrouping than phonetic ones. .... The proposal here presented is that the subclassifier should first concentrate his effort on the finding of morpholexical innovations, and if some innovations in different domains of the grammar are in conflict, the morpholexical ones should be considered decisive. It follows that morpholexical innovations are not expected to be contradictory among themselves, apparent contradictions may be due to borrowings." [30]   As can be gathered from the preceding quotations, the principle of 'shared morpholexical innovations', advocated by Hetzron, is essentially concerned with the subgrouping of related languages rather than reconstruction *per se*. Still, it should be remembered that a correct, noncontroversial subgrouping of related languages constitutes the very premise for any attempt at recovering unrecorded ancestral protolanguages.

As indicated above, some of Hetzron's claims and considerations have been presented previously, though perhaps less explicitly, by linguists working on the reconstruction of protolanguages and the subgrouping of genetically related languages; some of them were cited by Hetzron himself. To quote a further instance, the Polish linguist Tadeusz Milewski, when discussing the method of internal reconstruction, characterized it as follows (here by and large rendered in an English translation made by me earlier and for a different purpose): "The method of internal reconstruction (part of which is the establishment of relative chronology) is based on the presence of parallel elements in a language of a given period. By analyzing the system of that language, it ascertains which elements are older and which younger. This method exists in several variants. The first of them is based on inferences drawn from phonological parallels ..... For historical morphology, investigating the evolution of the morphological system of a language, the *method of exceptional forms* [italics added, H. B.] is of particular importance. This method is founded on the assumption that whenever we are confronted by two synonymous forms, a regular one corresponding to the normal, general morphological type of the language, and a second form which is anomalous and exceptional, then the latter of these must be considered a residual form and hence the older of the two.

30 See Hetzron 1976: 98-9.

Related to this method is that concerned with *fading forms*
[italics mine, H. B.] , implying that if of two synonyms one is
receding while the other is developing, the former must be
older." [31]   As should be readily clear from the passage just
adduced, the techniques proposed by Milewski, and notably
the 'method of exceptional forms', bear great resemblance to
those described by Hetzron (especially, his principle of 'archaic
heterogeneity'); the method of ascertaining and evaluating
'fading forms' is, of course, akin both to Hetzron's principle
of 'shared morpholexical innovations' and, even more so, to
Watkins' (and, in a sense, also Hoenigswald's) insistence on the
importance also of 'negative innovations'. [32]

Essentially, what we are concerned with here is of course the
very nature of linguistic change at the various levels of linguistic
structure. When we project such change far back into the past,
namely, into periods of unrecorded protolanguages or non-
attested earlier phases of subsequently recorded languages, it is
difficult, if not impossible, to take into account the —often
barely noticeable — adjustments, only ultimately and cumula-
tively amounting to full-fledged changes of a linguistic sub-
system, which occur when language is transmitted from genera-
tion to generation. Some years ago, Henning Andersen, in a
seminal paper, drawing on synchronic as well as diachronic
data gleaned from Czech dialectology, proposed a model of
phonological change "which would make possible the under-
standing of structural innovations in the phonology of a homo-
geneous speech community. The model, which distinguishes
two logically different modes of change (abductive and deduc-
tive), helps clarify the essential difference between 'internally
motivated' change and change 'induced from without'." [33]

31 Cf. Milewski 1965: 138-9; see also Birnbaum 1970a: 92-3, with the further
comment: "One may add here that this last method [viz., of fading forms, H.B.]
can be applied to the field of so-called syntactic synonymy where frequent instances
of competing constructions, some becoming or about to become obsolete or un-
productive, others growing in productivity, are particularly suited for the application
of the method of internal reconstruction." For a more detailed discussion of syntac-
tic reconstruction, see section *b*, below. A general assessment of Milewski's book
can be found, e.g., in Birnbaum 1967.

32 Cf. Hetzron 1976: 96-7 (with fn. 14).

33 See Andersen 1973; the quote is from the summary preceding the body of
the text of his paper. For a discussion of a broader typological framework of linguis-
tic change into which 'abductive' change or innovation fits, see further Andersen
1974: esp. 25-6 and 41 summarizing his typologies of innovation in the content
and expression systems of language. In both systems (or planes) of language Andersen

Couched in broadly generative terms — in the sense that he operates with models of grammars, numbered 1 and 2, and their output, that is to say, with a set of rules generating specific sets of linguistic data — Andersen's analysis presented in a novel and precise fashion the kind of change termed by him 'abductive', i.e., such change where the output of grammar 1 serves as the input to grammar 2 which in turn yields a reinterpreted output 2, slightly, yet significantly, different from output 1. [34] While applied by Andersen strictly to phonological change, his model of 'abductive change' has not only already served as the theoretical framework for the discussion of other instances of sound change, but, conceivably, can shed new light on the principles of linguistic change in general, thus also at other levels of linguistic structure, morphology being one of them. In this context it is worth pointing out that the understanding of the principle of 'abductive change' is by no means altogether new even though Andersen undoubtedly is the first to have explicitly formulated it in detail and done so using a broadly generative approach to the functioning of language. In essence, Hetzron, in the article referred to here at some length, has — presumably independently of Andersen (whom, at any rate, he does not cite) — stated the same principle, albeit not in generative but rather in old-fashioned 'taxonomic' terms, when writing: "Language is a structured system indeed. In diachrony, however, what is transmitted from generation to generation is not the structure, but a set of data which is analyzed by the child acquiring the language so that he could establish a structure for his own use. Language change is precisely justified by the fact that a subsequent generation may analyze the facts perceived by learning the language from the older generation in a manner somewhat different from the system of the older generation, and this may eventually require some adjustment in the facts, some modification of the perceivable data." [35] There can be no doubt that both Andersen's model of 'ab-

distinguishes between 'adaptive' and 'evolutive' innovations, with the former, in the content system, subclassified into 'accomodative', 'remedial', and 'contact' innovations; and in the expression system, into 'remedial,' and 'contact' innovations; the 'evolutive' innovations are subdivided by the author into 'deductive' and 'abductive' in both systems of language, with the 'abductive' innovations of the expression plane further subspecified as pertaining either to the phonemic system (where he distinguishes between (a) feature valuation, (b) segmentation, and (c) ranking) or to pronunciation rules.

34 Cf., in particular, figures 1 and 2 (Andersen 1973: 767 and 778).
35 Cf. Hetzron 1976: 96.

ductive change' and Hetzron's less elaborate formulation of the
same principle capture a very basic facet of linguistic change
even though the transmission of a linguistic system or sub-
system (or a grammar or grammatical component generating
this system or subsystem) from one generation of speakers to
the next must not be conceived of in all too rigid, mechanistic
terms since the distinction of successive generations in any real
speech community is never very clear-cut and easily ascertain-
able.

6. Syntactic reconstruction

Turning now to linguistic reconstruction at the syntactic and
semantic levels (both pertaining to grammatical — sometimes
referred to as 'function' — and lexical meaning and the relations
and configurations obtaining among meaningful units and
categories), it is fair to say that in recent years some significant
new avenues have been opened to these fields, previously
considered particularly inaccessible as far as reconstruction
is concerned.

For syntactic reconstruction, the distinction between pre-
linear deep structure and linear (sequential) surface structure is
of particular significance. For while it is rather generally
acknowledged that syntactic surface structure is highly suscept-
ible to foreign influences so that the syntax of a given language
under certain circumstances can be quite radically remodeled
by adaptation to patterns of another (neighboring, more
sophisticated and therefore more prestigious, or otherwise
influential) language, the same cannot be said — or, at any rate,
certainly not to the same extent — about the underlying
syntactic and semantic structure of a language. Thus, as I had
occasion to point out in an earlier study, "the realization that
we can posit a typological deep structure layer underlying also
genetically related languages — even a more loosely related
group such as the Indo-European languages as a whole — may
provide us with further important insights as to some general
characteristics and tendencies of Indo-European syntax. It can
also shed some new light on the occasionally claimed high
susceptibility of the syntax of one language (as compared to
other components of the structure of that language) to
influence from syntactic models and patterns of other, likewise
Indo-European languages." And I went on to suggest that "as

applied to a set of genetically related ...... languages, one could, for example, venture to say that much of the deep structure underlying the surface data of most, of the recorded ancient Indo-European languages displays a considerable degree of uniformity, thus suggesting a new, promising approach to the problem of reconstructing Proto-Indo-European syntax." For, while "superficially seen, syntax has correctly been assigned a high degree of 'penetrability' (by comparison with phonology and morphology), it can ......, in its typological deep structure aspect, actually be considered a fairly stable component of language also when viewed in the context of bilingualism and language interference." 36

It goes without saying that the task of reconstructing the syntax of an unrecorded protolanguage is substantially easier if that protolanguage is not separated by any sizable time gap from the first written testimonies of one or more of its daughter languages. Such is the case, for example, with Slavic. Here, the time intervening between late Common Slavic and attested Old Church Slavic (as well as early Old Russian) is a few centuries at most. Some scholars —myself included — have expressed the view that, for all intents and purposes, Old Church Slavic (of the 10th-11th centuries) can even be considered merely the recorded form of a late Common Slavic dialect (thus, perhaps the term Proto-Slavic may be preferable for explicitly denoting the entirely unattested protolanguage of the Slavs). Under these circumstances it was only natural to assume that, prior to realizing the implications of a stratified view of syntactic structure (assuming several — language-specific, typological, and language-universal — deep-structural strata beneath the readily observable surface-structural data), an attempt to winnow out both the striking as well as less readily discernible syntactic Grecisms from the syntax (or, rather, what now would be labeled surface syntax) of Old Church Slavic could reasonably be expected to reveal a good deal of the (surface-)syntactic structure of unattested Common Slavic (Proto-Slavic). Such expectations would not be unreasonable, particularly if analogous structures could be ascertained in other early recorded Slavic languages, notably Old Russian, where, at least in some of its genres, syntax could be assumed

36 See Birnbaum 1970a: 42-3, with further references to earlier writings of mine on this topic.

to have remained virtually uninfluenced by Greek, directly or through the impact of Old Church Slavic. As it turned out, however, such a procedure was ultimately somewhat less successful than at first anticipated, and the addition of a deep-structural dimension to Old Church Slavic syntax, where genuinely Slavic and Greek characteristics could not always be strictly separated, subsequently further complicated the overall picture while at the same time elucidating some previously poorly understood syntactic peculiarities of Old Church Slavic. [37]   Yet the possibility to more fully recover the syntactic structure of the Common Slavic protolanguage clearly remains within the reach of future research. [38]

Problems of reconstructing Common Slavic syntax were viewed in a broad, integrated context in a thought-provoking study by Vjačeslav V. Ivanov and Vladimir N. Toporov which discusses some general principles applicable to the recreating of entire Common Slavic text segments. As the two Soviet linguists state in the English summary of their paper, "it is possible to describe the process of generation of a Proto-Slavic text in two ways: by constructing a synthetic model where a text is generated from the upper levels (starting with the content) to the lower levels (such as morphological and phonological) or by building an analytic model starting from the lower levels." Choosing, for practical reasons, the second alternative and introducing special notational systems for each level, they describe the syntactic level "by means of a set of rewriting rules," where several constraints on the application of cyclical rules are taken into account. The separate "transformational level is described by means of a set of equivalence relations between different syntactical constructions that express the same syntactical meaning (that is, the meaning-preserving mapping of syntactical constructions is made the basis for the transformational level)." [39]   Concerned, to a large extent, with the extrapolation of hypothetic linguistic structures of varying length (in terms of their linear extension) from archaic folkloric literary and subliterary data, the Soviet

37 Cf. Birnbaum 1958/74, 1968a, and 1968/74.
38 Cf. Birnbaum 1975a: 180-99 and 292-8; for a general discussion of the problems and methods of reconstructing Common Slavic syntax, see, in particular, also Bauer 1963/72.
39 See Ivanov &Toporov 1963: esp. 157-8. Cf. further also Ivanov & Toporov 1965: 218-39 ("Nekotorye fragmenty rekonstruiruemyx slavjanskix tckstov").

linguists' theoretically intriguing suggestions are, however, only of limited value for the concrete reconstruction of Common Slavic syntax.

Having tested a new approach with a view to retrieving the phrase and sentence structure as well as the semantic or, rather more generally, semiotic system of Common Slavic, Ivanov, a specialist in Comparative Indo-European Linguistics, subsequently tackled the far more difficult and problematic task of seeking to reconstruct some of the basic syntactic structures of Proto-Indo-European, much farther removed in time. [40] After pointing out, in the mid-sixties, that the possibility of recovering some of the features characteristic of Indo-European syntax by comparing archaic syntactic structures of individual Indo-European languages had been established in the preceding two decades, the Soviet comparatist expressed the opinion that such reconstruction could be achieved with relatively most success in those instances where the syntactic phenomena were closely intertwined with phenomena of other, "lower" strata of linguistic structure: the phonological level (cf. syntactic accentology, whose study as applied to Proto-Indo-European was begun by Jacob Wackernagel toward the close of the last century), the morphological level (cf. the distribution of lexical items, or 'word-forms'. according to word class, and the analysis of morphological structures reflecting earlier syntactic structures), and the lexical level (cf. the etymological identification of words fulfilling a syntactic function). It should further be noted, Ivanov tells us, that inferences with a relatively high probability can be drawn from nontrivial syntactic data for when it comes to phenomena of a universal or near-universal nature, historical comparative — or, as we now rather would call it, genetic — linguistics turns into purely typological linguistics. [41]   In this context, I may refer to my own repeated claim that genetic language groups ("language families"), including Indo-European as a whole, can also be defined in typological terms, i.e., as constituting particular linguistic types, in this instance characterized by a number of shared structural retentions that survived the process of divergence from which the related ("daughter") languages resulted. As for the hierarchical relationship between typology

40 See Ivanov 1965: 185-289 ("Rekonstrukcija sintaksičeskix struktur").
41 Cf. Ivanov 1965: 185.

and linguistic universals, I would like to reiterate my previous suggestion "that language universals can be defined by a procedure implying a 'generalization of' − typological − 'generalizations'" and that "much of those phenomena and processes, now still considered universal, will, upon closer scrutiny, turn out to be typological by nature .... rather than truly universal, i.e., characteristic of language as such." [42]

Subsequently, Ivanov goes on to state that the strategy sketched by him leads to the paradoxical situation that heretofore the most credible inferences pertaining to the reconstruction of Indo-European syntax were arrived at on the basis of syntactic structures which because of their seeming exoticness at first glance appeared non-Indo-European and were even attributed to some non-Indo-European substratum. Such was the case, for example, with the Celtic infixed pronouns inserted into the verb complex (and mistakenly ascribed to Berber substratum by Julius Pokorny) and with the Hittite enclitic complexes cumulatively ordered at the beginning of the sentence. [43]   Applying his own approach, Ivanov discusses at some length the sentence-initial conjunctions of Indo-European which can be traced to original pronouns, analyzes the initial position of adverbs in sentences with the verb at the end (i.e., SOV, OSV), elucidates the nominal origin of adverbs/prefixes, along with the archaic type of Indo-European sentence structure, and then turns to the problem of original adverbs functioning as prefixes and pre- or postpositions. A section on pronominal elements in "Wackernagel's position" (i.e., occurring as enclitics in the second place within the sentence as defined by Wackernagel's law) is followed by one on autosemantic words found in the second position within the sentence-initial complex and on some problems of syntactic accentology. A subsequent chapter elaborates on the reconstruction of the schema projecting sentence trees with the verb in initial position, i.e., with reversed word order (VSO, VOS), and the concluding chapter comments in some detail on traces reflecting the archaic type of the nominal sentence in Indo-European. In sum, there can be no doubt that Ivanov's exploration of the syntax of Proto-Indo-European as it can be re-

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42 Cf. Birnbaum 1975c: esp. 23; 1975d: esp. 237-8. See also my reasoning along roughly the same lines in Birnbaum 1970a: 40-48.
43 See Ivanov 1965: 185-6, with fnn. 1-4.

covered with the aid of techniques applied by him with keen insight and due circumspection, though not making any claims to being complete or even in some regards systematic, marks a new, major advance in the difficult field of syntactic, or, to be more precise, surface-syntactic, reconstruction in this relatively best attested and researched area of genetic linguistics. It can therefore only be hoped that we soon will see the appearance of the volume on syntax, entrusted to him, in the multi-volume new Indo-European reference grammar under the general editorship of Jerzy Kuryłowicz and published by Carl Winter Universitätsverlag, Heidelberg. In a more recent, supplementary study, Ivanov has further discussed the evolution of pre- and postpositions in Indo-European and particularly in Slavic from a typological point of view. [44]

Winfred Lehmann, with his book on Proto-Indo-European syntax, has set for himself a more ambitious task as far as systematic coverage and, for that matter, modern methodology are concerned. [45] Though acknowledging that his *"Proto-Indo-European Syntax* is itself only a preliminary work," the American Indo-Europeanist and Germanist nonetheless professes that his goal is to remedy one particular shortcoming of Indo-European linguistics. This drawback is said to be manifest, on the one hand, in the fact that the findings of a long gone generation of Indo-Europeanists, to the extent they were concerned with syntax, are now obsolete as regards both data and method; this applies, in particular, to such earlier Indo-European syntacticians as Berthold Delbrück and Jacob Wackernagel who did not yet know the evidence of Hittite and Mycenaean Greek (in Linear B) and who, as neogrammarians, could not be aware of a generative approach to language. On the other hand, in more recent times, despite the availability of the pertinent data of early Indo-European dialects and, according to the American comparatist, the still ongoing elaboration of an apposite methodology, namely generative theory, Indo-Europeanists have primarily been preoccupied with what Lehmann — in this respect strictly following 'standard TG theory', only recently espoused by him, not 'generative semantics' — considers the "subsidiary components of grammar," that is to say, phonology and morphology. [46] Lehmann's work

44 See Ivanov 1973a.
45 See Lehmann 1974.
46 Cf. Lehmann 1974: ix (in the Preface).

therefore "attempts to provide an explanatory syntax for the parent language of the Indo-European language family" ....., "incorporating data unavailable to previous generations of linguists and taking advantage of an increased understanding of language in general and of syntax in particular." Lehmann has "based his description on generative theory" of the 'standard' type and "has made use of typological findings concerning VO and OV languages." [47]

In this connection, it is somewhat embarrassing to note Lehmann's obvious ignorance of Ivanov's work on reconstructing Proto-Indo-European syntax which, of course, fully takes into account the relevant data of Hittite, [48] Tocharian, Mycenaean Greek, (the latter two, however, being of lesser significance for the recovery of the syntactic patterns of the Indo-European protolanguage), in addition to Vedic Sanskrit, Homeric Greek, archaic Baltic, etc., while at the same time being quite au courant with contemporary linguistic theory. Also, Lehmann is mistaken, I would contend, when he suggests that "syntax is generally held to be the heart of grammar." [49] Rather, this view is consistently held only by Noam Chomsky and the adherents of his 'standard theory' and not necessarily by other generativists, especially not by generative semanticists or those linguistic theoreticians who see in generative grammar only one, albeit important, of several conceivable approaches to the study of language. In this context, it should further be remembered that Chomsky first developed and tested his ideas, as well as subsequent variants of his theory, on English language data; and modern English, by contrast to ancient and preliterate Indo-European with the latter's rich inflectional system, is a language whose relative morphological simplicity is compensated by a rather complex and sophisticated syntax, particularly in its surface manifestation (word order, means of subordination, etc.).

In addition to adopting 'standard' generative theory, with its implications for diachronic linguistics, Lehmann's treatment of Proto-Indo-European syntax also takes into account newly gained insights of language typology, in particular as they were

47 Cf. the statement in the blurb of the dust jacket of Lehmann 1974.
48 Yet the American linguist is aware of and refers to Ivanov's textbook of Hittite; see Lehmann 1974: 259.
49 Cf. this formulation in the blurb of the dust jacket of Lehmann 1974; for a more elaborate argument, see *ibid.* : 6-13.

formulated in Joseph Greenberg's writings on universals of word order. [50]   This, in my view, has undoubtedly added to the value of Lehmann's reconstructions. And even though he, at the outset, emphasizes the significance of the early Indo-European dialect data for reconstructing the parent language, it is in fact only the evidence of the earliest attested language of that family, Hittite (from around 1500 B. C.), that adds substantially to the general picture of Indo-European sentence structure of the preliterate period. Otherwise, our notion of it is still based primarily on the testimony of Sanskrit, particularly in its early variety of the Vedic hymns, ancient Greek, especially as it has come down to us in the archaic form of the Homeric epics, classical (and Old) Latin, as well as the early attested or archaic (if only late recorded) Indo-European languages and dialects many of which Lehmann draws on heavily. He cites the four processes of syntax defined by Leonard Bloomfield (a linguist of a persuasion very different from that of the modern generativists and other 'mentalists' who style themselves descendents of Edward Sapir and his conception of language) — processes which, incidentally, neatly correspond to certain syntactic means considered already by Hermann Paul, the codifier of the neogrammarian doctrine, to wit: selection, arrangement (order), modification, and modulation (intonation, sandhi). Lehmann points out that "for an understanding of PIE syntax, the role of each of these processes must be related. A sentence in PIE is generated by selection of word classes and inflected subsets of these arranged in various patterns, which are subject to sandhi changes and delimited by specific patterns of intonation." [51]   And, qualifying and limiting the aim of his undertaking, the American linguist subsequently states that "reconstructions are made under the assumption that PIE was a changing language much like those languages for which we have texts. I do not assume that it was consistently OV, especially in its later stages. Further, the syntax described here represents only one stage in the development of the reconstructed language from which the various dialects developed." And although his "syntactic description is by no means complete ...., the majority of the constructions are described." [52]

50 Cf. Greenberg 1964 and 1966.
51 Cf. Lehmann 1974: 24-5.
52 *Ibid.*: 26.

In the ensuing chapters, Lehmann treats, in this order 1)
the syntax of simple sentences and their surface patterns, with
the (finite) verb as the chief constituent; 2) nominal modifiers,
largely introduced by embedding achieved by deep-to-surface
structure transformations; 3) verbal modifiers and comple-
ments, again, to some extent, introduced by embedding, in
addition to the means of coordination; 4) syntactic categories
used in the construction of PIE sentences, discussing coordina-
tion and subordination (embedding), as well as the syntactic
devices of substitution and the use of function words, congru-
ence (grammatical agreement) and government; 5) lexical
entries, as they relate to elements expressed in grammatical
categories (in this respect, thus strictly adhering to syntactical-
ly-based generative theory); and, finally, 6) the evolution of
syntactic patterns in some IE dialects, with particular attention
paid to changes entailing a shift from OV to VO sentence
structure.

As can be gathered even from this brief survey of the subject
matter covered in Lehmann's book, his is indeed a serious
attempt to fairly systematically apply a generative approach
to syntactic reconstruction, aiming at a full and explicit specifi-
cation of both the presumed underlying (deep-seated) and the
surface syntactic structures. The relative length at which I have
dealt here with Lehmann's ambitious endeavor, synthesizing a
number of his preliminary specialized studies which are general-
ly well underpinned by relevant linguistic data, should serve to
indicate my appreciation of his novel exploration of this
territory, some strong reservations notwithstanding. Apart from
ingoring Ivanov's important relevant work, noted previously,
these reservations pertain primarily to 1) the purely hypothetic,
and thus questionable, nature of the underlying syntactic
patterns (deep structures) posited by Lehmann, which are not
susceptible to empirical corroboration, and 2) the, in my view,
exaggerated significance attributed by him to the sequential
arrangement of surface structures (notably, the distinction
of OV vs. VO structures) for a language or, in any case, a
certain evolutionary phase of a language which, typologically,
by all we can surmise about it was highly flexible as to word
order. (The latter was probably determined by considerations
of emphasis, foregrounding, topic/comment-contrasting, and
the like — in other words, phenomena and processes subsum-
able under the notion of 'functional sentence perspective' and,

perhaps, syntactic stylistics rather than syntax proper.) The grammatical functions in other, including many modern, languages, fulfilled by means of shifting a rigorously fixed word order (now usually symbolized VSO, SVO, SOV, corresponding to Greenberg's types I, II, III), were expressed in Proto-Indo-European above all by strictly morphological, primarily inflectional, means.

Lehmann's preliminary studies on (Proto-)Indo-European syntax which lead up to his monograph have prompted a response of sorts in the form of Paul Friedrich's booklet bearing the same title as the Texan scholar's book, with, to be sure, a qualifying subtitle echoing a formulation used by Greenberg. [53] Friedrich assumes a set of variables in reconstructed Proto-Indo-European as well as in recorded early Indo-European word order — with each variable, once implemented, entailing, as an implicational rule, a certain fixed order of other, secondary elements (so-called non-dominant order, of the type AN, GN, SA, PA = advective + noun, genitive + noun, standard-adjective order, pivot + adjective, etc.). He further emphasizes the particular significance of Homeric Greek, said not to have been heretofore sufficiently considered. His polemic paper opens with a half dozen basic conceptual questions, to wit: 1) What do the variables mean? 2) What are the hierarchical and teleonomic relations between the variables? 3) What is the proper relation between typology and reconstruction? 4) [What is the heart of] the problem of evidence and of argument? 5) What is the comparative evidence for PIE syntax? 6) [How is one to assess] Homeric Greek and Proto-Indo-European syntax? After forcefully arguing his points, Friedrich, in recapitulating his argument, notes that most of it was devoted to examining the relative value of the case for Greenberg's type II (SVO) and type III (SOV). "To this end [he] intensively analyzed Homeric Greek, reviewed the comparative material and earlier literature, did summaries of the Old Armenian, Old Iranian, Hittite, and, in particular, the Tocharian systems, and finally, reinstated Italic, Old Irish, and Proto-Slavic to the place which they merit in the comparison of early Indo-European syntax." [54]   Friedrich thinks that he has shown "the limitations of the 'OV hypothesis', and that it is just as

53 See Friedrich 1975.
54 Cf. Friedrich 1975: 67-8.

fruitful to posit a type II for Proto-Indo-European. In other words, the OV hypothesis is still only *a hypothesis*, and *not a tested theory* [author's italics, H. B.] ." [55]   In two methodological postscripts, he then discusses some general issues which have been overlooked, misinterpreted, or insufficiently elucidated by diachronic/synchronic generativists in their attempts to reconstruct syntactic systems, in particular, the basic indeterminacy in syntactic derivation. Friedrich further examines possible flaws also in Greenberg's universalist models of syntactic structure and, especially, word order. It seems to me that the points he raises against some of the claims made by advocates of diachronic generative theory are generally well taken. Also, his plea for a more loosely structured, 'gestalt' model of syntax has some merit. In sum, therefore, I feel that Friedrich's, to be sure often sharply phrased, criticism and his line of argument certainly deserve to be taken into account in any future attempts at reconstructing the syntactic patterns of the Indo-European protolanguage. Though he is almost exclusively concerned with surface syntax — as opposed to Lehmann who pays about as much attention to underlying structures as to their assumed surface manifestations — I would suggest that Friedrich has demonstrated the invalidity of any claim that Proto-Indo-European sentence structure was primarily, if not exclusively, of the (S)OV type, a view ultimately going back to Delbrück.

Another, earlier contribution to Indo-European sentence structure is Calvert Watkins' paper presented at the Ninth International Congress of Linguists wherein he seeks "to establish certain equations in phrase structure among certain of the older IE languages: Vedic, Hittite, Latin, and Old Irish." While based on solid linguistic data and adopting Jan Gonda's keen observation that "the grammatical procedures by which words and word groups were arranged and united into meaningful larger units were often highly conventional," Watkins' study essentially operates only with devices of the traditional comparative method and, as pointed out by me in the discussion, covers, despite its promising title, merely "a limited problem of word order in IE verb phrases" treated, incidentally, at greater length by Watkins elsewhere. [56]

55 *Ibid.*: 68.
56 See Watkins 1964: esp. 1035 and 1044-5.

Concerning the application of syntactic analysis outside the field of Indo-European linguistics, though primarily not for the purpose of recovering a common lost protosystem but rather to seek to corroborate (or refute) claims of distant genetic relationship between two language families, Uralic and Altaic (suggesting the erstwhile existence of a linguistic 'macrofamily', which, in the final analysis, presumably would represent the result of divergent evolution from, as it were, a twice removed protolanguage), see, below, in section 8.

7. Semantic reconstruction

As regards linguistic reconstruction at the semantic level, my comments will focus, in some detail, on a few problems only. Generally, it can be said that the retracing and recovery of the lost meaning inherent in lexical items which can be assumed to have existed in unrecorded protolanguages or preliterate stages of subsequently attested languages is of course, methodologically, no easy undertaking, especially when it comes to words which allow for more than one semantic (and/or stylistic) shade or connotation. Also, it goes without saying that if the exact borderline separating linguistic (or more specifically, lexical) semantics from the semantics of symbolic logic is not always easy to draw for language in its synchronic and contemporary aspect where the object of investigation lies open to immediate observation, experimentation, and analysis, the task of identifying and properly describing meaningful units, including an analysis of their makeup in terms of constituent elements (or 'semantic features'), in the diachronic aspect of language, is incomparably more difficult even in such instances where we are dealing with some period (or periods) of the past for which written — or otherwise perceptible, e.g. chiseled or carved — evidence is available. To infer from such evidence semantic structures (and their expression) of yet farther removed, unattested periods of linguistic evolution entails, obviously, a large measure of hypothesizing and operating on a probabilistic scale. However, having voiced this necessary note of caution and having emphasized the need for the greatest possible circumspection, I should also point out where semantic reconstruction is on relatively safe ground. Thus when it comes to items of what can be considered the basic vocabulary (German *Grundwortschatz*, Russian *osnovnoj*

slovarnyj fond) of the reconstructed parent language, it is , as
a rule, possible to posit equivalent meanings for presumed
protoforms reflected, also as regards phonological correspond-
ences (i.e., Hjelmslev's '[expression] element function'), in items
of identical meaning in all or most of the daughter languages. In
cases of occasional additional or deviating meanings, the course
of secondary semantic shifts or splits usually can be retraced
with a high degree of probability. Cf., e.g., such lexical items
of many or most Indo-European languages as those for 'father',
'mother', 'son', 'daughter'; 'water', 'fire'; 'heaven', 'earth'; 'sun',
'moon'; and quite a few others; but cf., by the same token, also
early — though usually retraceable — semantic shifts of the kind
represented by, say, Gmc *beran* (Goth *baíran*, E *bear*, Sw *bära*,
etc.), Sl *berǫ*, *bьrati*, Lat *fero*, Gk φέρω < PIE *bher-*, and the
like.

At the microstructural level, then, semantic reconstruction
is part and parcel of one particular subdivision of historical
comparative (genetic) linguistics, namely, etymology. In
addition to considerations of diachronic semantics, etymolo-
gical research must, as is well known, also resort to the findings
of historical phonology and, in particular, derivational morpho-
logy (or rather, more generally, the theory of word formation
in its historical aspect). Any new advances in the field of
etymology, with respect to data as well as to theory and
method, signify therefore also a measure of progress in the area
of semantic reconstruction. This is not the place, however, to
comment, other than in the most general terms only, on some
spectacular recent achievements of etymological research in
various languages and language groups. [57]

Such, also methodologically significant new insights into the
semantics of reconstructed phases of linguistic evolution have

[57] In a very general fashion, we can refer here to a number of new or newly
begun etymological projects, many of them resulting in the publication of updated,
revised, or even altogether newly conceived etymological dictionaries. To cite just
a few examples from the branch of linguistics most familiar to me: at present there
are at least four research centers, each engaged ipublication of an etymologically
founded Common Slavic or Comparative Slavic dictionary: one in Graz (and
Würzburg), headed by Professors Linda Sadnik and Rudolf Aitzetmüller; one in Brno,
previously headed by Professor Václav Machek and now directed by Professor
František Kopečký; one in Cracow, headed by Professor Franciszek Sławski; and one
in Moscow, directed by Dr. Oleg N. Trubačev. The Moscow etymological research
center (forming part of the Russian Language Institute of the Academy of Sciences
of the U.S.S.R.), in addition, has been issuing, since 1963, an annual publication,
Ètimologija, devoted to Slavic, Indo-European, and general etymology.

been gained primarily at what may be termed the level of semantic macrostructures, by contrast to a microstructural — or, as regards individual lexical items, even 'atomistic' — level. By semantic macrostructure I will denote here any set of conceptually interrelated items (categories) of some magnitude and complexity, either on the paradigmatic or the syntagmatic axis of language. Theoretically, the largest conceivable such paradigmatic set would reflect the whole of reality surrounding man as well as any abstraction and imaginary concepts derived from it. It is such a maximal semantic set that would call for taking a clear, unequivocal stand as to the validity (or lack thereof) of the main thrust of the so-called Sapir-Whorf hypothesis. That is to say, we would have to decide whether to accept or reject the notion that man's perception of reality (and its imaginary and theoretical corollaries) and, specifically, his ability to discern its components and subcomponents ultimately are determined, not to say imposed, by the (semantic) structure of language or, rather, of any one particular language used by man, or, at any rate, by primitive man. In other words, the heart of the Sapir-Whorf hypothesis constitutes a linguistic theory of relativity of sorts; for fairly recent discussions and reassessments of this hypothesis, see, e.g., Gipper 1972 and Pinxten 1976. In this context it should be noted that the view according to which it is language that, to some extent, shapes our perception of reality (and its projection into the unreal and/or transcendental), and not vice versa, that the natural structuredness of the universe surrounding us in some way reflects on the manner in which we structure our language (especially at the semantic level), represents, in a sense, the very opposite extreme of the view found in the kind of linguistic mentalism on which generative theory (and Noam Chomsky's notion of language in particular) is based while both, remarkably enough, claim a considerable share of debt to the linguistic thought of Edward Sapir. [58]

On the syntagmatic axis, the largest conceivable semantic set would consist of the sum total of all texts ever generated in a particular language (assuming here, for the sake of argument, one specific synchronic stage of that language). In effect, however, such a maximal set must be conceived of as open or infinite for it is always possible to add, now or in the synchro-

58 Cf. also Birnbaum 1975c: 24; 1975d: 238-9 and 243-4, n. 17.

nically acceptable future, further texts written or spoken in that
language. Smaller syntagmatic semantic sets would consist — on
a decreasing scale — of one single text, a segment of discourse,
one single utterance, sentence, or clause. It is when we come
down to these smaller linear units, and particularly to the
sentence, that we can speak of an interaction (and interface)
between semantic and (deep) syntactic structure, with the
former informing the latter (cf. also the discussion in section
1, above). Therefore, a successful reconstruction of some
fragments of surface-syntactic structure (especially word order,
i.e.; the sequential ordering of meaningful units, cf. section 6,
above), which would allow for certain inferences as to the
categories and relations obtaining in prelinear (nonsequential)
deep-syntactic structure, can in turn prove useful also for
semantic reconstruction, particularly in its syntagmatic (linear)
dimension.

Drawing the ultimate conclusions from this kind of theoriz-
ing would lead us too far afield, however. Rather, what we will
be concerned with here are some attempts, undertaken in recent
years, to recreate, as it were, a particular segment of reality
(and/or its imagined extension) as well as its prehistoric seman-
tic articulation (structuredness). The examples adduced will be
taken from the Indo-European language family as a whole and
from one of its branches — Slavic, the field of my own primary
competence.

Perhaps the most ambitious and impressive endeavor in this
vein, covering   the whole range of Indo-European, is the
magnum opus of one of the most outstanding disciples and the
worthy academic successor of the unforgettable Antoine
Meillet, the two-volume monograph *Le vocabulaire des institu-
tions indo-européennes* by Émile Benveniste, crowning and,
unfortunately, concluding the meteoric career of this brilliant
Indo-Europeanist. [59]   To sum up the aim and achievement
of this truly unique (if at times controversial) study, it may be
proper to quote the capsule statement found on the back of
the cover of this work as formulated or at least inspired by the
author himself:

Dans cet ouvrage, la méthode linguistique comparative
est employée à un dessein d'ensemble: l'analyse du vocabu-
laire propre aux grandes institutions dans les principales

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59 See Benveniste 1969; for a discussion of the particular method, see also
Benveniste 1957.

langues indo-européennes. Partant des correspondances entre les formes historiques, on cherche, au-delà des *désignations*, qui sont souvent très divergentes, à atteindre le niveau profond des *significations* qui les fondent, pour retrouver la notion première de l'institution comme structure latente, enfouie dans la préhistoire linguistique. On jette ainsi une lumière nouvelle sur les fondements de maintes institutions du monde moderne, dans l'économie, la société, le droit, la religion.

Following a brief preface on scope and method, the two volumes are each subdivided into three "books" treating: 1) *l'économie*, 2) *le vocabulaire de la parenté*, 3) *les statuts sociaux*, and 1) *la royauté et ses privilèges*, 2) *le droit*, 3) *la religion*. Without entering into the details of this magnificent piece of research which elucidates the chief economic, consanguineal, social, political, legal, and religious institutions with their internal organization and rules as reflected in the relevant nomenclature of early, prehistoric Indo-European society on the basis of the corresponding residual terminological systems preserved (or echoed) in historical, i.e., linguistically attested, times, it can be said that the French comparatist has indeed succeeded — a few remaining moot points notwithstanding — in the task he had set for himself. By using the traditional, if modernized, comparative method (while, in addition, taking recourse also to the findings of Indo-European archaeology, mythology, folklore, etc.), Benveniste has uncovered and thus recreated a central portion of the Proto-Indo-European vocabulary, not only in its earliest reconstructable phonological shape, but also as regards the original, underlying meanings that can be assigned to each of the pertinent lexical items to the extent that their semantic prehistory can be retraced on the basis of available comparative, in part vastly divergent, early Indo-European data.

It may be argued, perhaps, that, at least as compared to the evidence adduced from other Indo-European languages, the data of Slavic (and Baltic) cited in Benveniste's monograph is somewhat scanty. This also applies to kinship terminology, traditionally a favorite domain of etymological research. However, as regards Slavic kinship nomenclature, this part of the vocabulary was the subject of two specialized studies, both published in the 1950s. One is the article by Alexander V. Isačenko (1953/76), where the Slavic subset of relevant designations is tied in with its Indo-European cognates and semantic

counterparts and where, as pointed out by me elsewhere, etymological ingenuity and erudition are combined with socio-linguistic (and sociological) insights and empathy. [60]   On theoretical grounds, Isačenko concludes that the Indo-European noun (substantive), as had once alreday been suggested by Jan Rozwadowski, was originally made up of two elements of which one functioned as the determinant of the other. The precise meaning and grammatical status of these elements (e.g., *tēr, *sue, *sor in the discussed instances), according to Isačenko, cannot always be ascertained for the most remote periods of Indo-European linguistic evolution. As to sociological infer-ences, the author, on the basis of the data analyzed by him, posits an original matriarchal structure for earliest Indo-European society, a conjecture which, however, perhaps does not withstand close scrutiny (cf. below).

A few years after the first appearances of Isačenko's study, Oleg N. Trubačev published his monograph on the history of the Slavic terms of kinship and some other ancient terms per-taining to social structure (Trubačev 1959). While Isačenko's relevant essay can be considered fairly systematic, if not entirely free of some less well-founded hypothesizing, Trubačev's monograph on the related, though somewhat more broadly conceived topic can be said to be truly exhaustive as regards both the phenomena and problems discussed and the secondary literature consulted. In addition to the treatment of the terminology of blood relationship proper in chapter one, a second chapter is devoted to the nomenclature of conjugal affinity, and a further, third chapter examines designations connected with kinship terminology as well as some early terms used with reference to the structure of the tribal clan and society in general. One point where Trubačev was able, it seems, to refute an intriguing conjecture by Isačenko has to do with the interpretation of the IE suffix *ter-/*-ter-/*-tr-. Isačenko, on the basis of such formations as Gk ἔντερα, OCS ǫtroba, denoting inner organs (cf. also Lat extra, intra), assumed an original adverb meaning 'inside, within' which, supposedly, would have given rise to the suffix *-ter/*-tē(r) found in Indo-European kinship designations. However, as Trubačev has

60 See Isačenko 1953/76; the reprinted version (in the author's *Opera Selecta*) is somewhat shortened, eliminating some of the previous references to Marxist linguistics; for a brief appraisal, see also my few remarks in the preface to the volume of Isačenko's selected essays, *ibid*.: 8; and Birnbaum 1975a: 207.

pointed out, this particular meaning is inherent only in the root morpheme *in-/*en- (with futher ablaut variants) whereas the element *-tr-/*-ter- here merely plays the role of a specifying formant (affix). 61   Like Isačenko's earlier study, Trubačev's thorough investigation is firmly anchored in the tradition of the comparative method as it has been applied to the Indo-European language family. However, while Isačenko in addition to purely linguistic considerations also introduced a measure of sociological speculation (not entirely unaffected by Marxist doctrine), Trubačev is more concerned with an etymological-structural analysis of an entire major segment of the Common Slavic and pre-Slavic lexicon. This particular approach has also led him to some general conclusions regarding the derivational facts and features pertinent to this portion of the Indo-European and Slavic vocabulary.

A similar holistic method of semantic reconstruction, this time combined with a 'Wörter-und-Sachen' type of approach, was used by Trubačev in his subsequent monograph on the terminology of crafts in Slavic (Trubačev 1966). Treating in three chapters the terminology of textile (especially weaving) manufacture, woodworking, and crafts connected with the use of fire (pottery and smithery), and citing folkloric and literary fragments relevant to these crafts in an appendix, the Soviet etymologist arrives at a number of significant conclusions, extra-linguistic as well as linguistic. Among the former is the realization, supported by evidence, of the close correspondence between the historical data of material culture and that of language. Further, he was able to trace the origin of pottery in plaiting. Of purely linguistic (and ethno-linguistic) insights worth mentioning are 1) the inadequacy of artisanal terminology resulting from its archaizing nature (characteristic, it should be added, of all traditional, folk nomenclature); 2) the secondary — i.e., relatively late — "occidentalization" of the two Sorbian languages (now spoken in some communities in southern East Germany); 3) early lexical ties between Serbo-Croatian and Ukrainian (as well as Belorussian) or, rather, the Common Slavic dialects underlying these languages; and, perhaps the most surprising result, 4) the quantitative ranking of pertinent lexical isoglosses shared by Common Slavic with other early Indo-European dialects, namely, the preponderance of Slavic-

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61   See Trubačev 1959: 193; cf. also Isačenko 1953/76: 60-1/77-8.

Germanic and Slavic-Latin lexical agreements (in this order) over Slavic-Baltic conformities in the vocabulary, thus suggesting a chronology different from that heretofore usually assumed as regards the prehistoric ties between Baltic and Slavic. [62]

Benveniste's, Isačenko's, and Trubačev's semantic reconstructions, resting on the solid foundations of etymological erudition and ingenuity coupled with recourse to the findings of other, related disciplines, though modern and up-to-date, are basically applications of the traditional comparative method in historical linguistics to a particular, well-defined domain of language. A somewhat different, bolder and more far-reaching, but admittedly also more problematic approach to the recovery of specific meanings and combinations of meanings which are not readily apparent was adopted by the two Soviet linguists Vjačeslav V. Ivanov and Vladimir N. Toporov. Theirs may be called a text-modeling and relational method of semantic reconstruction. Continuing the line of research begun with their jointly authored congress contribution (Ivanov & Toporov 1963; cf. above, with n. 39), the two comparatists have in two more recent monographs (Ivanov & Toporov 1965 and 1974) investigated and sought to reconstruct some main figures and features of Proto-Slavic (and partly Common Baltic) mythology, viewed in their general Indo-European setting. The bulk of the earlier monograph deals, in two major sections, with "some fragments of the Slavic model of the world" (where the primary religious system, i.e., the Slavic pantheon proper, is scrutinized), and with "the question of reconstructing the content plane of the ancient Slavic religious system." The authors draw on recent structural interpretations of comparative Indo-European mythology and anthropology and apply the method of semiotic modeling now rather widely used by Soviet humanistic scholarship (linguistics, literature, culture). Incorporating into their theoretical framework some of the semantic and typological insights of glossematics and availing themselves of suggestions made by Roman Jakobson concerning Slavic mythology, philology, and etymology, they give a systematic interpretation of the functional peculiarities and relations obtaining among the Slavic pagan deities, to the extent their names have come down to us (sometimes in distorted

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62 On Trubačev's relevant research, including some preliminary work see further Birnbaum 1975a: 207-8, 211, 307-8.

form). As a result, Ivanov and Toporov advance some general ideas bearing on the reconstructed language of the prehistoric Slavs and on the presumed world view which it reflects. They then provide a semantic analysis of some fundamental antinomies which are all conceived of as a subset of semantic distinctive features, as it were, indispensable for describing the earliest Slavic religious system. Subsequently, the possibility of describing this system at different levels of semantic generalization/specialization is explored and some typological facets of the early Slavic religious system are discussed. Though perhaps promising even more in its title, Ivanov's and Toporov's earlier monograph on this topic captures an important segment of Slavic mythology and reality as linguistically conceptualized in a distant, preliterate past and, by accomplishing this, marks a major advance in the elaboration of a new set of techniques for semantically elucidating and identifying a central portion of the vocabulary of a lost, reconstructed language. [63]

The second, more recent monographic study by the same authors (Ivanov & Toporov 1974), while in its title indicating the general area of investigation, suggests in its subtitle that the two Soviet linguists, again, are primarily concerned with "lexical and phraseological problems of text reconstruction." Further refining the method of semiotic modeling, this monograph consists of two parts: one attempting to reconstruct fragments of the myth about the thunder god and his adversary; the other discussing some transformations of that basic myth and the corresponding ritual in connection with the complex notion of fertility. The individual chapters deal with the East Slavic *Perun(ъ)* in the framework of reconstructing Proto-Slavic, Baltic, and Proto-Indo-Eruopean texts about the thunder god; with the East Slavic *Veles (ъ)/Volos(ъ)* and the problem of reconstructing the name and attributes of the adversary of the thunder god; with the duel between the thunder god and his adversary; with the problem of reconstructing the ritual underlying the myth about this duel; with the typological and areal interpretation of the myth about the duel between the Thunderer and the Serpent; and with the transformation of the heroes of the basic myth. The chapters of part two treat the idol *Jarila* (or *Jarilo*) and his equivalents among the Slavs; the comparative study of some Slavic rites connected with the

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63 Cf. further also Birnbaum 1975a: 209-10.

idol (and festival of) *Kupala*; the symbolism of *korovaj*
('wedding cake, uncut loaf of bread' etymologically a cognate
of Russian *korova* 'cow') in connection with the origin of the
korovaj rite; and the typology of systems using binary classi-
ficatory features. As can be gathered even from this brief
listing, this monograph is concerned with the restoration of a
basic hypothetic text from which a number of secondary
texts are derived. This basic text bears on the primary para-
meters of the spiritual and material life of the ancient Slavs.
In its fragments can be found essential terms whose lexical
and phraseological reconstruction is elucidated in the
etymological portions of the book. Here, also other ancient
Indo-European traditions are discussed as well as the relevant
evidence from two adjacent areas, the Baltic territory and
the Balkans. This broad linguistic and cultural context must
by now be considered a necessary prerequisite for solving the
controversial problem of the ethnogenesis of the Slavs and that
of Slavic antiquities. Particular attention is paid in the book
to the various transformations of the basic text as they were
determined by the subsequent cultural and historical conditions
which affected this text. By the same token, the consideration
of the various transformations of the basic text which previous-
ly were viewed in isolation now allows for a substantial increase
in the number of — secondary — sources establishing many new
details of the basic text. Typological parallels from other tradi-
tions contribute to the ascertaining of chronological, areal,
social, and cultural characteristics of both the hypothetic basic
text and its concrete transformations. Typological data of
binary oppositions, tabulated in matrix form and assessed as to
their role in linguistics and related disciplines, can be used to
more precisely identify the place of the Proto-Slavic semantic
complex among other cultural-linguistic traditions. [64]

This fairly detailed account of some recent achievements in
Slavic (and Indo-European) etymology and semantic reconstruc-
tion was given here to illustrate the degree of methodological
sophistication and interdisciplinary competence required to
further advance our knowledge in this area. [65]   In particular,

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[64] For some further information on Slavic mythology, see, e.g., Gimbutas
1971: 151-70 and 187-8, 194-5 (references).

[65] For some further research along these lines by the same scholars, see, e.g.,
Ivanov & Toporov 1973 (on etymologizing semantically well-defined, limited fields
of the Common Slavic vocabulary); Ivanov 1973b (on language as a source for ethno-
genetic inquiry and the problem of Slavic antiquities); Toporov 1975 (on Slavic

the significance of mythological and folkloric data also for purposes of linguistic, and especially semantic, reconstruction cannot be emphasized enough. Naturally, what was adduced here from the field of Slavic linguistics (and related disciplines) holds true, in principle and *mutatis mutandis*, also for other branches of Indo-European linguistics and, I would think, for some further areas of comparative linguistics, though perhaps with differently distributed emphases, as well.

8. *Distant genetic relationship and typology: toward the reconstruction of 'preprotolanguages': the case of Nostratic*

So far we have discussed linguistic reconstruction, its methods and levels, applicable to one particular sample family of related (viz., the Indo-European) and closely related (viz., the Slavic) languages only. Let us now consider various, earlier as well as recent, attempts to uncover genetic ties linking two or several well-defined language families (Indo-European, Semitic, Hamitic, Kartvelian, Altaic, Uralic, etc.), which would suggest the existence, in some distant past, of language families larger than the traditionally established ones, i.e., encompassing two or more of these generally recognized families and thus constituting what may be termed linguistic 'macrofamilies'. Here the question arises: should these 'macrofamilies', too, if indeed they can be ascertained or, at any rate, posited with a high degree of probability, not be traced back to a common origin, that is to say, to a parent language which, by contrast to what usually has been labeled 'protolanguage', tentatively may be referred to as 'preprotolanguage'? If some such 'preprotolanguage' can actually be assumed to have existed in some remote period of linguistic evolution, it is only natural to then also pose the further question: is there any way in which such a 'preprotolanguage' or at least some portion thereof (component, level of its structure) can be recovered, either in concrete linguistic terms — sounds, forms, phrases, etc. — or in a more abstract fashion, as a linguistic model which could serve as a theoretical point of departure from which subsequent, albeit early but more concretely reconstructible linguistic developments can be derived? And, as a corollary to this latter

mythological terminology and possible Near Eastern parallels); and, on a different semantic field, words denoting gift and exchange, Benveniste 1951/66 (for Indo-European in general) and Ivanov 1975 (for Slavic in particular).

question, the problem of distinguishing between genetic and typological linguistics reappears. This time, however, this problem is not so much the fundamental theoretical issue of hierarchical ranking in language grouping (typology over genealogy) and of using typological insights in genetic diachrony (linguistic reconstruction, and, though with significant qualifications, also linguistic prediction, presupposing typological generalizations and implications), discussed above (in section 3), but rather the pragmatic problem of how to establish what correspondences and conformities qualify merely as typological (structural) isomorphisms and what other ones point to genetic — to be sure, distant genetic — relationship as well; in short, where does one draw the line between typology and genealogy in what by some linguists has cautiously and noncommittally been termed 'external (language) comparison'? In what follows, I have not striven for any systematic coverage of relevant phenomena and problems but have merely tried to illustrate some of them with a few telling examples, citing recent pertinent literature where in turn references to earlier treatments can be found. We will first consider some work examining the possibility of establishing distant genetic relationship between two traditional language families; thereafter we will take a brief look at a recent renewed attempt to define a broadly conceived, comprehensive 'macrofamily', and conclude this section with a few remarks concerning both the nature of the 'preprotolanguage' which, if the notion of a genetically distantly related language 'macrofamily' is accepted, must be assumed to underlie this later vastly diversified set of language groups and, finally, some conceivable, in part already tested techniques for recovering certain facets of this long vanished linguistic structure.

As is well known, the existence of a distant relationship between the Uralic and the Altaic language families has long been considered possible, though not unanimously accepted. However, it would seem to me that the arguments supporting such a genetic relationship, drawn in particular from morphology (inflectional as well as derivational) and from lexicology — agreements which made one of the leading experts in the field, Björn Collinder, consider such a relationship reasonably likely 66 — can be substantially corroborated also by syntactic

parallels which are not easily explicable as due to mere coincidence. This was with much insight and circumspection ascertained and analyzed by David R. Fokos-Fuchs. [67] As for a conceivable distant genetic relationship between Uralic and Indo-European, such an assumption can, given the present state of our knowledge, neither be positively proven, nor can it be definitely refuted; only in the light of a much larger, potential 'macrofamily', Nostratic (to be discussed below), which would include not only Uralic and Indo-European but also several other traditionally established language families, can the 'Indo-Uralic' hypothesis gain additional support. [68]

Similar considerations apply to another conceivable distant relationship, that between Indo-European and the Kartvelian (South Caucasian) language group. Thomas V. Gamkrelidze, in a series of publications discussing Kartvelian-Indo-European parallels, especially as regards morphophonemic alternations (of the ablaut type), has so far only cautiously assumed typologically striking analogies (interpretable, perhaps, in terms of an "extensive" *Sprachbund* of the kind once suggested by Roman Jakobson). [69] However, here, again, an integration into a wider framework of distantly related language groups, Nostratic, may allow for a reinterpretation in genetic terms also of the Kartvelian-Indo-European linguistic relationship. Arguments of an analogous nature, quantitatively even exceeding the structural and lexical similarities found to exist between Kartvelian and Indo-European, can be adduced when re-examing the 'Indo-Semitic' hypothesis. Recent work by A. B. Dolgopol'skij (who, in addition, has investigated common features in the structure of the root morpheme of Semitic and Hamitic) and, in particular, Allan R. Bomhard — with his

lich") and 136-55; see also Collinder 1964: esp. 157-202 (originally published in 1947 and 1950).

67 See Fokos-Fuchs 1962.

68 Cf. Collinder 1964: 17-155 (originally published in 1934, 1943, and 1954); further, Collinder 1965: esp. 109-36; and, most recently, Collinder 1974: esp. 363-70. In this context should also be noted Collinder's objection (1965: 174, n. 36) to Louis Hjelmslev's claim that "there can be no doubt of the genetic relationship between Indo-European and Uralic after Holger Pedersen's success in demonstrating a number of element-correspondences"; cf. Hjelmslev 1970: 80 (in the original Danish version of Hjelmslev's book *Sproget*, published in 1963, the quote can be found on p. 77).

69 Cf. Gamkrelidze 1966: esp. 83, 1967: esp. 218, 1970a, 1970b: esp. 141, 1971: esp. 2: 28 ; 3: 34-48. On R Jakobson's now somewhat dated notion of an "extensive" (i.e., essentially phonological) *Sprachbund*, e.g., that of Eurasia, see Birnbaum 1965: 15-17; 1966: 18; 1968b: 71 and 89 (n. 2).

ample references to previous relevant research — who has studied analogies in the consonant systems of Proto-Semitic and Proto-Indo-European, Indo-European vowel gradation and comparable phenomena in Semitic phonology, as well as a number of grammatical similarities and shared nominal and verbal stems, suggests that the affinities obtaining between these two language families may well go beyond mere typological parallelism and could be viewed as ultimately pointing to a common origin in hypothetic 'preprotolanguage'. [70]

Finally, and before turning to the as yet only hypothetic Nostratic 'macrofamily' and its parent language, it should be noted that Indo-Europeanists reconstructing the Proto-Indo-European sound and form system (but hardly its syntactic or semantic structure) have occasionally also applied considerations which, by a procedure that may, in a sense, be termed 'internal reconstruction' (in that it uses the Proto-Indo-European data, arrived at by means of the comparative method and, presumably, only to a lesser degree by internal reconstruction, as the synchronic base for a further projection backward in time), were apt to retrieve an earlier linguistic structure of Proto-Indo-European (or a portion thereof) than that directly underlying the attested Indo-European languages. Perhaps, it would be more appropriate to label such an earlier system Pre-Indo-European, rather than early Proto-Indo-European, as in fact has been suggested by some linguists, for example, Carl Hj. Borgström and Allan R. Bomhard. [71]   This Pre-Indo-European linguistic system may therefore turn out to at least in part coincide with, or more closely reflect, what here has been referred to as Nostratic.

The resuscitation of the 'Nostratic hypothesis', which had

70 See Dolgopol'skij 1967 and 1970; Bomhard 1975: 355-74; and esp. Bomhard, forthcoming. Note, in particular, Bomhard's closing remarks: "Even though there are many differences between Semitic and Indo-European, I think that the similarities are far too numerous to be ignored. A new start has been made in joining together these two language families. The future of the 'Indo-Semitic' Hypothesis looks extremely exciting and promising."

71 See Borgström 1954; Bomhard 1975: 355-74. It should further be noted that Bomhard has attempted to reconstruct not only the phonemic system of Pre-Indo-European (see esp. op. cit.: 355) but, in addition, also those of consecutive stages of the Indo-European proto-language, labeling them 'the phonemic stress stage of Indo-European', 'the phonemic pitch stage of Indo-European', 'Late Indo-European', and 'Disintegrating Indo-European'; cf. Bomhard 1975: 374-87; see further also Bomhard 1976 (esp. sections 1-3) where he derives Proto-Anatolian from 'Pitch Indo-European'.

been originally, in the beginning of this century, tentatively advanced by Holger Pedersen, [72] was undertaken, above all, by two Soviet linguists, the prematurely deceased Vladislav M. Illič-Svityč (d. 1966) and the now emigrated A. B. Dolgopol'skij. Other Soviet linguists have cooperated with them in this endeavor and have, after Illič-Svityc's death, continued his work. Among them are V. A. Dybo (with whom Illič-Svityč had previously collaborated also in the field of Balto-Slavic linguistics, especially, accentology, and who is the editor of Illič-Svityč's major, posthumous work on Nostratic [73]), G. A. Klimov, L. A. Gindin, A. A. Zaliznjak, V. N. Toporov, and others. Illič-Svityč's synthetic, posthumous book on Nostratic is hopefully, working conditions permitting, soon to be followed by one or several sequel volumes to be written by Dolgopol'skij or a team of specialists headed by him. The future work will contain, among other things, a comparative grammar of Nostratic which, in particular, will treat phonology, morphophonology (morphophonemics), and morphology. In addition, the two linguists have authored a number of specialized studies dealing with various aspects of Nostratic linguistics, the member families as well as some particular facets of the posited underlying linguistic system or 'preprotolanguage'. [74] Even though it may still be too early to draw any final and definitive conclusions from the available evidence and the relevant research done to date, it seems safe to say that the data gathered and analyzed so far points to an extremely high probability that the six traditional language families concerned are indeed linked by genetic relationship which, in view of the considerable divergence resulting from separate developments over a vast geo-

72 The term 'Nostratic' was first proposed in Pedersen 1903: 560. It should be noted, however, that Holger Pedersen's definition of 'Nostratic' does not fully coincide with the more recently suggested one. According to Pedersen, the Nostratic 'macrofamily' considered by him tentatively comprised Indo-European, Hamito-Semitic, Uralic, and possibly the sub-Saharic language families Bantu and Sudanese; cf. also Hjelmslev 1970: 80.

73 Illič-Svityč 1971. Cf. also the extremely positive view by V. V. Ivanov, considering the book "one of the highest achievements not only of linguistics but, generally, of the humanities in the 20th century" (Ivanov 1974: esp. 184); see further also the guardedly positive assessment by such a circumspect scholar as B. Collinder (1974: 370-2).

74 Cf., in particular, Illič-Svityč 1964, 1967a, 1967b, 1968a, and 1968b; Dolgopol'skij 1964a, 1964b (with a variant version), 1965, 1967a, 1967b, 1969, 1970, 1971, 1972, 1974 (and probably further relevant studies not yet available to me).

graphic territory and the substantial length of time elapsed since the first breaking up of these subsequently separate language groups, can be best qualified as 'distant'. Thus three degrees of genetic relationship among languages can be distinguished: 'closely related' (Russian *blizkorodstvennye*, German *nahverwandte*), 'related', and 'distantly related' languages; analogically, we could speak of three kinds of genetic language groups: 'microfamilies', 'families', and 'macrofamilies'. These three degrees of genetic relationship can be exemplified by Slavic (or any other branch of Indo-European, constituting a 'microfamily'), Indo-European (or any other branch of Nostratic, constituting a regular language family), and Nostratic. Obviously, however, there are some degrees of relationship not covered by these three standard ranges. Thus, on the one hand, any subgroup within a closely related language family, say, East Slavic within Slavic or Nordic (Scandinavian, North Germanic) within Germanic, also forms a closely related group, but one that is even more closely related than the specific language family of which it forms a part. On the other hand, there are also transitional degrees between closely related and plainly related languages, e.g., within Indo-European, Balto-Slavic or Indo-Iranian, or, within Uralic, Finno-Ugric, with further subdivisions of Finnic into Baltic (or West) Finnic and Volga (or East) Finnic, etc.

It seems clear, at any rate, that the agreements and conformities found among the Nostratic languages cannot be simply and exhaustively explained in terms of typological isomorphism or linguistic borrowing even though many details of early linguistic evolution in Eurasia, contemporaneous with major ethnic upheavals and migrations and a nomadic or semi-nomadic way of life conducive to large-scale linguistic borrowing, have not yet been sufficiently elucidated. Distant genetic relationship is, therefore, the by far most likely hypothesis. As pointed out by the editor of Illič-Svityč's posthumous work, V. A. Dybo, the chief result of the late linguist's research is not only the definite establishment of the fact that the six compared reconstructed protolanguages (Proto-Indo-European, Proto-Uralic, Proto-Kartvelian, etc.) share a certain number of semantically and phonologically similar morphemes (lexemes) which form regular sets of correspondences but, in addition, also his producing the proof that these regular sets of correspondences together form an elaborate system of equivalents

into which the protosystems of the Nostratic languages can be fully integrated, without leaving any significant lacunae. The gaps of typological comparison (contrasting) in the phonological systems of the Nostratic protolanguages are filled — i.e., eliminated — by the establishment of genetic identity. Thus, the resulting system of correspondences represents a rigid system of converting one phonological system into another one. The arbitrary nature of the vocabulary (i.e., the independence of the phonological relations from lexical selection or, to be precise, from the selection of lexical correspondences) and the systemic nature of the sets of correspondences preclude any suggestion of the outcome being fortuitous as well as any assumption of mutual lexical borrowing. The intersecting or cross-overlap of the conversion systems makes it impossible to explain a number of lexical items as being the result of borrowing from one of the six Nostratic language families and calls for the introduction of an intermediary system (or model). The introduction of such an intermediary linguistic system resolves any existing contradictions in the conversion systems and renders them uniform by creating a unified, monodirectional system of conversion. Naturally, this intermediary system does not itself coincide with any of the six protolinguistic systems. Thus, three possibilities are theoretically conceivable: 1) that the intermediary system is the underlying system of one of the six protolinguistic systems ; all comparable data of the other five protolinguistic systems would then derive from that one underlying system (the 'preprotosystem' of the sixth protolinguistic system); 2) that the intermediary system is identical with a partially reconstructible vanished, or in its descendants not yet revealed, protosystem (substratum) from which all comparable data of all six protolanguages derives; 3) that the intermediary system is identical with a reconstructible protolanguage from which all six compared protolanguages have naturally evolved. In choosing from among these three conceivable alternatives, one cannot, of course, consider the phonological correspondences alone. In addition, the lexical and morphological systems of the protosystem to be reconstructed must be taken into account. Though many specific meanings of lexical items can be established only approximately or tentatively, the most basic meanings — the items of the core vocabulary — are represented to a rather large extent in that segment of the lexicon which has already been recon-

structed. The set of reconstructible formants (affixes) constitutes a fairly large segment of the morphological system and the possibility that such a segment could have been borrowed simuntaneously — and *en bloc*? — by five (alternative 1) or six (alternative 2) other linguistic protosystems would imply an unprecedented instance in the history of language. These considerations, therefore, render alternatives 1) and 2) highly unlikely while the third solution, suggesting that the reconstructed general protosystem itself represents a protolanguage in relation to each of the six hypothetic protolanguages underlying the traditionally established language families (i.e., that it constitutes a 'preprotolanguages' in the terminology adopted here), assumes a high degree of probability. [75]

In light of the above, the 'Nostratic hypothesis' can now be considered fairly well founded, with, to be sure, many concrete details yet to be ascertained and elaborated. It should also be noted, in this connection, that some linguists of great erudition who are not known to be overly prone to unfounded speculation have, with some qualifications, espoused this bold and truly revolutionizing hypothesis which was adumbrated, in vague outline, by Holger Pedersen nearly a century ago and which since has been and is being further tested and elaborated by a group of brilliant linguists following in the footsteps of the much too early departed Vladislav M. Illič-Svityč. Björn Collinder's generally positive and receptive attitude, causing him to use even poetic language when speaking of this new, exciting prospect, was already noted above; likewise Vjačeslav V. Ivanov's appraisal in superlative terms. [76]   Reference should further also be made here to what Raimo Anttila has to say about 'distant relationship' in general and Nostratic and 'cross-Bering comparisons' in particular. [77]   Finally, I might mention that I, too, have earlier ventured a cautiously positive assessment of the 'Nostratic hypothesis', pointing, especially, to the merging, if not of genetic and typological relationship,

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75   Cf. Illič-Svityč 1971: xxxiv-v (authored by V. A. Dybo).

76   See note 73, above. Cf. further Collinder 1965: 172: "Aber den Zweiflern möchte man mit Orest zurufen: 'Lass dir raten, habe die Sonne nicht zu gern und nicht die Sterne; komm folge mir ins dunkle Reich hinab!' " And also Collinder 1974: 372: "Es weht ein frischer Wind über die Steppe hin."

77   Cf. Anttila 1972: 320-1; see further, e.g., Swadesh 1965. For some possible distant genetic relationship among Amerindian languages, see now, e.g., Goddard 1976.

then at any rate of genetic and typological considerations in the definition of such broadly conceived language groupings or 'macrofamilies' as Nostratic. 7 8 And I will readily concede that I am today even more inclined to accept the notion of distant genetic relationship, and not only of typological affinity, as far as the Nostratic languages are concerned.

In concluding the discussion on distant genetic relationship and Nostratic, we may add a word or two concerning the value and relevance of establishing such distant genetic relationships, and of at least partially recovering such long vanished linguistic structures as that of Nostratic, for the methods of linguistic reconstruction. Basically, it may be argued that such attempts at recreating fragments of a purely hypothetic 'preproto-language' of the distant past do not, in fact, add any new theoretical insights readily applicable to a further refining of our present techniques of reconstruction. For, to be a bit more specific, also when we seek to establish and identify the sounds, forms, and meanings — and in some future time perhaps even some of the prelinear ('deep') relations and linear ('surface') arrangements of these forms and meanings, that is, portions of the syntax — of such a linguistic system, far removed in time, as Nostratic, we resort, primarily, to the well-established method of comparative reconstruction, only with the difference that the data on which we base our reconstruction is, in many cases, much less reliable and certain as it, too, is merely reconstructed and hence hypothetic. In addition, procedures of internal reconstruction would be applicable here as well: treating the more or less certain data of each of the reconstructed protolanguages (Proto-Indo-European, Proto-Uralic, Proto-Kartvelian, etc.) as separate sets of synchronic data, we can proceed to project, as it were, each of these sets backward in time. Thus, from the morphophonemic alternations, the co-existent and/or competing subsets of forms and, possibly, even fragments of syntactic structure we can draw inferences as to preceding stages in the development of the protolanguages which, the protolanguages themselves being the result of reconstruction (in turn based on the evidence of the extant daughter languages), represent the final phase of their erstwhile homogeneous, as yet undifferentiated existence (if, in this context and for the sake of the argument, we disregard any conceivable

78 Cf. Birnbaum 1975c: 12-13; 1975d: 228-9.

spatial-dialectal variation). Cf. what was said above regarding the possibility of recovering, by internal reconstruction, earlier evolutionary stages of Proto-Indo-European or, rather perhaps, a phase which appropriately could be termed Pre-Indo-European. (Similar, if in general less precise and detailed, reconstructions could be achieved for Pre-Uralic, Pre-Kartvelian, etc.)

However, while work on reconstructing a 'preprotolanguage' such as Nostratic perhaps will not contribute any substantive new theoretical insights since such reconstruction, too, must primarily resort to the established methods of comparative and internal reconstruction, the very comparison of the Indo-European (Uralic, Kartvelian, etc.) language family with other, distantly related language families, i.e., with the other Nostratic language groups, can, pragmatically, open up new avenues and approaches also to the reconstruction of Proto-Indo-European itself (or, for that matter, to that of any other of the Nostratic protolanguages). For, clearly, in the still unresolved, controversial issues or uncertainties concerning the Proto-Indo-European system – as a couple of the many examples remaining within the relatively best researched field of prehistoric phonology, one might mention the question of the precise number and nature of the PIE laryngeals or of the PIE velar series – a comparison with other, but (distantly) related language families may very well suggest some decisive factors or considerations. Thus, the wider or 'external' comparison of all the Nostratic languages can indeed have repercussions and bring new insights and a better understanding also to the reconstruction of the generally assumed protolanguages. By contrast to internal and straight comparative reconstruction, such more far-reaching comparative reconstruction, when applicable to one traditional language family only (Indo-European, Uralic, Kartvelian, etc.), could be labeled 'external reconstruction', a term otherwise occasionally reserved for reconstruction based on extra-systemic linguistic (loanword, onomastic) data or on extra-linguistic (archaeological, historical, botanical, etc.) evidence.

9. Concluding remarks: Linguistic change and reconstruction

As we approach the conclusion of this essay on current problems and methods of linguistic reconstruction, it may be

useful to consider, once more, the relationship obtaining between the nature of linguistic change and the possibilities and prospects of reconstruction. In doing so, we will have occasion to refer to some — obviously not all — pertinent work done in this field during the sixties and seventies.

There can be no doubt that Henry Hoenigswald, in his monograph treating precisely and exclusively this subject matter (Hoenigswald 1960), already pointed to some crucial problems and potentials in our understanding of the very nature of language change preliminary to elaboration of methods and procedures with the goal of recovering lost language structures. Yet his own approach at the time was largely dictated by a post-Bloomfieldian concept of language which he, erudite comparatist as he is, was able to combine with the generally accepted tenets and achievements of Indo-European (and other) comparative linguistics, whose roots go all the way back to neogrammarian tradition and before. Thus, as he stated in the preface of his book, his effort was focused on "developing from the fundamental notions of synchronic linguistics the framework in which the recognized processes of change take their place — not as entities different in kind but as special instances of the wider process of replacement and merger (with its corollaries)." And he went on to point out "that formal reasoning based on a comparison of contrasts is fruitful even in the absence of uninterrupted documentation" and that his aim was "to make that reasoning more explicit and to provide a unified formal language for its various phases." [79]   The three notions — contrast, replacement, and merger — run as a leitmotiv throughout Hoenigswald's book, which is rich in factual detail, keen observation, and thoughtful interpretation. Language, while subject to change, is viewed by him consistently as something given *a priori*, a datum, affected by time, that is, changing diachronically (with, to be sure, the seed for such change lying in the tensions and dynamics of synchrony). But nowhere in Hoenigswald's presentation is language conceived of as something different as well: as the product of an ever-changing grammar, or conglomerate of rules, anchored in — partly innate, partly acquired — competence of the speaker (and hearer; secondarily, the writer and the reader) and conditioned and distorted merely by the interference of speaker-

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[79] See Hoenigswald 1960: v-vi.

internal (and hearer-internal; mental and/or motoric) and
speaker-external (and hearer-external; situational, social)
factors which are responsible for the greater of lesser dis-
crepancy between competence and performance. In other
words, when Hoegsniwald wrote his book, the view was pre-
dominant, as is well known, that the linguist, also the historical-
ly oriented linguist, should be concerned primarily, if not
exclusively, with describing and analyzing, as meticulously,
precisely, and exhaustively as possible, the linguistic data at his
disposal, but not that he should also draw from this data
any particular inferences concerning the grammar that had
generated it, let alone the reasons for, and the mechanics of,
the replacement of 'grammar 1' by 'grammar 2', etc.
Hoenigswald is therefore beyond reproach for not having
considered and adopted this — broadly generative — approach.
To avoid any possible miscomprehension, let me state again that
I, for one, do not hold that a generative approach to language
and, in particular, language change ought to be the only legi-
timate one, to the exclusion of other approaches. To the con-
trary, I feel that generative grammar (in one of its more reason-
able varieties) can, no doubt, contribute to, and hence broaden,
our understanding also of linguistic change and thus, moreover,
provide some additional powerful tools for linguistic reconstruc-
tion (cf. Henning Andersen's work referred to above). But it
cannot, in my opinion, replace earlier, time-honored methods
which, after all, have supplied us with most of the data base
of Indo-European linguistics (as well as that of some other
language families; cf. further also sections 0 and 1, above).

Oswald Szemerenyi, in his inaugural lecture delivered at the
Universtiy of Freiburg/Br., tackled, with many new and keen
observations, the problem of reconstruction and typology,
previously with insight and incisiveness briefly touched upon
by Roman Jakobson (1957/71), among others; Szemerényi
applied his considerations primarily, though not exclusively,
to Indo-European and its "new look" proposed. by him
(Szemerényi 1967). After surveying previous discussions on the
impact of typological implications and constraints on the
methods of linguistic reconstruction and commenting critically
on some of them, the Hungarian-born German Indo-Europeanist
sketched, in his usual brilliant, concise, and insightful fashion,
a "new look" of both Pre-Indo-European (defined by him as the
"period preceding IE and distinguished by certain features from

IE") and (Proto-)Indo-European (where "IE means the linguistic stage which can be reconstructed from the data of the IE languages as their immediate antecedent") phonology in a "first attempt to present an integrated and typologically acceptable view of the IE phonological system" whose salient points are (here quoted verbatim from his summary which hardly allows for any further condensation): "In IE there is only one laryngeal, the glottal fricative *h*. The current doctrine that Pre-IE had only one vowel is false. Pre-IE, like IE, had the full complement of the classical five-vowel triangle *a e o i u*. Corresponding to these short vowels, the IE languages also have phonemic long vowels. The thesis that all monsyllabic roots had a central core, *e*, flanked on either side by one consonant, is false. The trilateral structure of such roots is certainly not universal. Besides *e, a* and *o* also appear as root-cores. Besides the generally recognized Mediae Aspiratae there were also Tenues Aspiratae. The phonemic status of the former is therefore no longer in doubt." As can be seen from this concise statement, Szemerényi in his reconstruction of the Pre-Indo-European and Proto-Indo-European sound systems (where, as far as the stops are concerned, he takes another view than, e.g., Ivanov and Gamkrelidze or Hopper and Bomhard; cf. above) does not limit himself to phonology proper alone but also presents his view of the basic characteristics of the Indo-European root structure. Clearly, Szemerényi was well aware of the seminal nature of his paper when he stated that "this paper is merely a beginning which must be followed by a renewed investigation of the problems here reopened for discussion." [80]   This call has certainly not gone unheeded (cf. also below).

In his subsequently published textbook of comparative linguistics, again concerned primarily with Indo-European (Szemerényi 1970), he has integrated some of his new view of the Indo-European and Pre-Indo-European phonological systems and has considerably elaborated on it; in addition, he treats there at some length also Indo-European morphology, its history and prehistory. In an introductory chapter, he, moreover, demonstrates in a very graphic manner, invoking a simile taken from Plato's *State*, how historical linguistics and comparative linguistics, each taken in the narrow sense, first have to coordinate their efforts for the purpose of retriev-

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80 See Szemerényi 1967: 96-7.

ing lost (proto)languages and how they, after having attained
that goal to the extent possible, then must turn around, as it
were, and taking the reconstructed linguistic structure as their
point of departure, retrace, progressing in time, the evolution
that ultimately resulted in the preserved, current or historical-
ly attested, language form. [81]   However, Szemerényi regards
a realistic (in contradistinction to a merely abstract, formulaic)
reconstruction of the underlying common parent language as
the chief task of Indo-European linguistics. [82]

In his comprehensive textbook on historical and comparative
linguistics, which abounds in subtle observation, original
thought, and up-to-date bibliographic references, Raimo Anttila
devotes several chapters to the problems of reconstruction. [83]
Here, Anttila first discusses at some length the significance of
dialect geography as an indicator (or, rather, a set of indicators)
for linguistic reconstruction. Thus, it is pointed out that varia-
tion and spatial distribution of linguistic phenomena can
provide valuable clues regarding the diachrony of both attested
and unattested linguistic evolution. After a brief discussion of
the history of dialect geography and the more recent structural
dialectology, establishing, among other things, 'diaphonemes'
and whole 'diasystems', the Finnish-American scholar turns to
the important notions of stratigraphy and seriation. The two
methods, borrowed from geology and archaeology, have
significant applications also in linguistics, particularly in linguis-
tic reconstruction. Areal linguistics, a further development
and generalization of dialect geography, can, as Anttila demon-
strates, often establish the relative chronology of the two sides
of an isogloss line. Also, it turns out that a diasystem (or
'dialect cohesion') can be conceived of as a gradual transition,
i.e., feature by feature, and the wider the diasystem of dialects
or languages is geographically, the better the chances are for
capturing linguistic history and reconstructed prehistory. In a
subsequent chapter, Anttila shows how alternative relationship
models (family trees, wave theory diagrams, etc.) can be
integrated or synthesized, and their significance for the methods

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81 See Szemerényi 1970: 9; cf. also the chapter on linguistic change, *ibid*:
13-27.
82 See Szemerényi 1970: 28-30, esp. 29.
83 Cf. Anttila 1972: 287-374 ("Linguistic reconstruction: a synthesis of various
linguistic and cultural notions") and 377-88 ("Change and reconstruction in culture
and linguistics," esp. 383-7: "Reconstruction")

of linguistic reconstruction. Thereafter, the relationship obtaining between language classification (typological of various kinds, genealogical of various degrees) and reconstruction is examined, and the continued importance of philological and etymological research also for reconstruction is explored. Ensuing chapters discuss in some detail the actual procedures and techniques for reconstructing phonology, grammar, and semology/semantics. Here, Anttila treats, among other things, such problems and phenomena as matching and correspondence, the preventive impact of external forces, phonetic and internal reconstruction, and inverted — as opposed to internal — reconstruction. The chapter on phonological reconstruction ends with a brief discussion of analysis and analogy, and analogy and psychological reality. In the chapter on reconstructing grammar, the author first suggests a reinterpretation, for the purpose of reconstruction, of morphology as applied phonology and then discusses relative chronology and morphological types. The section on reconstruction in syntax accounts for the particular methodological difficulties in this area, illustrating some problems by exploring the relationship of Proto-Indo-European pronouns and Hittite connectives, and that of sentence connectives and the verbal categories of tense and mood. A few remarks on the indeterminacy of historical order (succession), suggesting that syntactic reconstruction, wherever possible, must heavily rely on morphology (and phonology), round out the chapter on grammatical reconstruction. In the particularly troublesome field of semantic reconstruction, Anttila first elaborates on the role of the comparative method in all — hence also in semantic — reconstruction and then goes on to discuss some of the relevant aspects, such as internal analysis of apparent synonyms, the application of Boolean algebra to semantic reconstruction, and the relationship of context, protoculture (mirrored in word meanings), and relative chronology of concepts.

From the preceding discussion it should be clear that Anttila, like others, considers the prospects of reconstruction best in phonology, less so (on a decreasing scale) in morphology, syntax, and semantics (cf. especially also Hamp 1974, discussed below). A further chapter on "change and reconstruction in culture and linguistics" focuses on some analogies which can be ascertained between cultural anthropology and linguistics; in the section on reconstruction, he discusses such problems,

methods, and areas of research as the comparative method in anthropolo gy, the historical method, seriation and stratigraphy, living paradigms in reconstruction, and migration theory. In closing this chapter, Anttila cites, to illustrate the complexity of problems and the methodological difficulties, a pertinent formulation by Edward Sapir to the effect that no single mode of historical inference can ever account for the complex and contradictory actualities of history.

Even from this fairly brief survey of the relevant chapters and sections of Anttila's content-packed text it should be apparent that his discussion covers a wide range of problems immediately relevant ·to linguistic reconstruction; what may have remained less than explicit in this account is that in a number of concrete instances he demonstrates the possibility of solving particular problems of reconstruction by applying the proper method (or methods), and identifies other instances where present procedures and techniques are as yet incapable of providing the necessary answers.

Without going into a detailed account, it ought to be mentioned here that in recent years some particular aspects of linguistic reconstruction have attracted much attention and led to proposals of in part new strategies for coming to grips with difficult, unresolved , or in some linguists' view, unsatisfactorily solved problems. Several such relevant contributions can be found in the two volumes of proceedings resulting from the First International Conference on Historical Linguistics held in Edinburgh on September 2-7, 1973. Henning Andersen's paper (Andersen 1974) was already mentioned previously (see fn. 33). In addition, a contribution by Daniel Gulstad on reconstruction in syntax (Gulstad 1974) discusses, from generative premises, several of the difficult facets of syntactic reconstruction. Claiming the fundamental unity of the synchronic and diachronic systems of language, or, in other words, that language is but one system with interlocking functions (even though the author considers it legitimate to focus on either the synchronic, 'horizontal time-line' or the diachronic, 'vertical time-line' aspect of language), Gulstad recognizes the significance of the logical structure for linguistic, especially syntactic, analysis without, however, assuming that the linguistic structure can simply be derived from the logical one. It is not realistic, Gulstad argues, to consider trivial all those changes that take place higher in linguistic structure than at the level of the logical

substructure. The observations and description of any structural changes, including syntactic ones, must be explained "in relation to the overall internal logic peculiar to the system of the particular language under examination." [84]

Some practical suggestions on how to use the computer in attacking problems of comparative reconstruction are briefly discussed by John Hewson (Hewson 1974) who outlines a strategy utilizing "the best possible interaction and integration of man and machine." The linguist's role is, presumably, two-fold: to write a program (algorithm) for the computer (or have such a program designed by a computer programmer following the linguist's directions) and to "direct his efforts to the challenging problems that remain when the purely mechanical and regular items have been cleared from the data." [85]

In his paper on "the major focus in reconstruction and change," Eric Hamp (Hamp 1974) surveys an impressive array of pertinent Indo-European examples and arrives at some general conclusions regarding linguistic change which, according to him, "does not occur equally or equally easily in all sectors" of language. To him, "there seems to be a hierarchy, ascending from the least to the most resistant: output-phonetic, phono-logical, syntactic, syntactic-semantic," with the last leading "directly out of language and into the structure of human culture." Two further claims that Hamp considers worth testing are: 1) that "any reconstruction or hypothesis of change should, in principle, favour or expect change to a greater extent, over an appreciable period of time and in proportion as dialect and language boundaries are crossed in the course of analysis, in the phonetic and phonological range than in the syntactic-semantic"; and 2) that "we may hope to come thereby to understand the reasons for observed regularities of linguistic change." For, "if change, sometimes starting randomly, makes its way through the above hierarchical spectrum from left to right, we may expect it to sweep the field and leave behind ultimate regularities in the leftmost ranges, while penetrating only unequally under complex conditions in ranges farther to the right." [86]   It should be noted that Hamp, in the hierarchically ranked 'ranges' of linguistic structure does not — presum-

84 See Gulstad 1974: 152.
85 See Hewson 1974: 197.
86 See Hamp 1974: 166-7.

ably following, in this respect, standard TG theory — posit any separate morphological 'range' which would intervene between phonology and syntax. Cf. also Anttila's suggestion to conceive of morphology as applied phonology (at least when discussing changes and reconstruction) and his similar view as to the prospects of reconstruction in the various components of language (see above). Hamp's (and Anttila's) relevant views are certainly worth pondering (and empirically testing), controversial as both the justification for omitting — or not recognizing an independent status for — morphology and the hierarchical acceptance/resistance ranking, as regards reconstruction, of the various levels (or components) of language must remain.

Returning to some problems of reconstruction, and especially internal reconstruction, Henry Hoenigswald (Hoenigswald 1974) argues for deemphasizing the division between internal and comparative reconstruction as well as the ordering of the two procedures in the sense that the former necessarily has to precede the latter. The possibilities of (internal) inference are limited and hence insufficient to retrieve unconditional, context-free processes. In what follows, Hoenigswald illustrates with primarily Latin and Germanic phonological data such phenomena, previously discussed by him, as splits (primary and secondary), mergers (including 'third-party mergers') and remerging, gap-filling, and analogy. All these processes are viewed, albeit informally, as expressible in the form of algorithms or quasi-algorithms. [87]   Of particular interest is Hoenigswald's claim that internal reconstruction is methodologically inadequate when it comes to recovering not only proto- but also preprotolanguages (or portions thereof). [88]

Oswald Szemerényi's suggestion and hope that his 'new look' of Indo-European might trigger renewed research in the field (especially Pre-Indo-European and Proto-Indo-European phonology; cf. above) has already begun to show results. In a sense, Gamkrelidze's and Ivanov's, as well as Hopper's, work (Gamkrelidze & Ivanov 1972 and 1973; Gamkrelidze 1972; Hopper 1973), discussed earlier (see section 4), was a response of sorts. More recently, Paul Hopper has, again, reexamined Indo-European consonantism and its 'new look' (Hopper 1977

87 Cf. esp. Hoenigswald 1974: 200, fn. 3.
88 See *ibid.*: 189. As shown with the example of Nostratic discussed above, both internal and comparative methods have to be applied in order to recreate some fragments of preprotolinguistic structure.

and forthcoming) and further elaborated on his previously advanced views, largely coinciding with those held by Gamkrelidze and Ivanov but very different from those of Szemerényi. And, word is out that the two Soviet linguists are currently putting the finishing touches on a new major monograph investigating "Indo-European and the Indo-Europeans" which is aimed at providing "a reconstruction and structural-typological analysis of the Proto-Indo-European language and the Indo-European culture" (Gamkrelidze & Ivanov, forthcoming). It can therefore be said that progress in linguistic reconstruction and theorizing on linguistic change is, at least as far as Indo-European is concerned, at an all-time high (matched perhaps only by the heyday of Indo-European studies in the fold of the Neogrammarian School). An overview and appraisal of some of the more significant problems and methods of linguistic reconstruction, its potentials and limitations, and of the impressive achievements reached to date, as have been attempted here, may therefore serve as a useful point of departure for future inquiry into the — for lack of direct evidence — concealed past of linguistic evolution which is only gradually and reluctantly yielding ground to the searching mind of the ever-more sophisticated and better equipped historian and prehistorian of language.

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CORRECTIONS AND ADDITIONS

Linguistic Reconstruction: Its Potentials and Limitations in New Perspective, Henrik Birnbaum.

p. 9[6,7]	"redundants (etic or emic)." should read "redundant (etic)."
p. 22[14]	"(p)" should be "(p̣)".
p. 22[15]	"voice" should be "voiced".
p. 23[7]	"T. Vennemann)[22] rule" should read "T. Vennemann)[22], rule".
p. 33[17]	"'word-forms'. according" should read "'word-forms', according".
p. 39[18]	"advective" should be "adjective".
p. 42, fn. 57, l. 5	"engaged ipublication" should read "engaged in publication".
p. 46[16]	"appearances" should be "appearance".
p. 48, fn. 62, l. 1	"work see" should read "work, see".
p. 50[2]	"bread' etymologically" should read "bread', etymologically".
p. 54[9]	"in hypothetic" should read "in a hypothetic".
p. 62[8]	"proposed. by" should read "proposed by".
p. 70[13]	"sudslavischen" should be "südslavischen".
p. 72[16]	"fonetičeskix sootvetstvij"" should read "fonetičeskix sootvetstvij)"".
p. 72[17]	"unavailable)." should be "unavailable]."
p. 72[18]	"obsceindoevropejskogo" should be "obščeindoevropejskogo".
p. 72[19]	"*Etimologija*" should be "*Ėtimologija*".
p. 72[21]	"isčut" should be "iščut".
p. 72[24,25]	"*soobscenija*" should be "*soobščenija*".
p. 72[19]	"*aout*" should be "*août*".
p. 72[15]	"*Etimologija*" should be "*Ėtimologija*".
p. 73[6]	"*Phonetica,,*" should be "*Phonetica,*".
p. 73[3]	"*obsčeindoevropejskogo*" should be "*obščeindoevropejskogo*".
p. 75[11]	"vnešgego" should be "vnešnego".

Corrections and Additions (2)

p. 75[18]   *"Etimologija"* should be *"Ėtimologija"*.

p. 75[24]   *"Mezdunarodnyj"* should be *"Meždunarodnyj"*.

p. 75[7]   "ětnogeneticeskix" should be "ėtnogenetičeskix".

p. 76[11]   *"semioticeskie"* should be *"semiotičeskie"*.

p. 76[13]   *"Etimologičeskoe"* should be *"Ėtimologičeskoe"*.

p. 76[17]   "N.Y.: H.R. & Winston" should read "New York: Holt, Rinehart and Winston."

p. 77[16]   "mifologiceskogo" should be "mifologičeskogo".

Addendum to page 55:

The second volume of the late Illič-Svityč's *Opyt sravnenija nostratičeskix jazykov* has meanwhile appeared as has a set of *Tezisy* summarizing achievements to date and future research goals.

General addendum:

Ė. A. Makaev's *Obščaja teorija sravnitel'nogo jazykoznanija* (Moscow: "Nauka", 1977) came to my attention only after the typescript of the present essay had been submitted to the Publisher. Discussing some pertinent data (esp. of Germanic and Armenian), the author also raises some theoretical and methodological issues relevant to reconstruction. I cannot agree with some of them, e.g., his objection to "mixing" the *ontological* and *gnoseological* levels for purposes of linguistic extrapolation.